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PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*On the Progress of Asiatic Cholera.* By ROBERT J. GRAVES, M. D.

(*Continued from* p. 316, vol. vi.)

SINCE the publication of my last article on this subject, cholera has reached England; and its arrival has been attended by circumstances, which make me quite sure that the opinion, so positively announced both here and in London, by the Board of Health and the College of Physicians, rests on a very doubtful foundation. Before I proceed, however, to arraign this opinion, it may be well to recapitulate it. The Board of Health in Dublin have published a circular, in which we find this passage: "The Commissioners of Health are anxious to impress upon all persons the important difference that exists between cholera and fever, with respect to the mode of propagation of these epidemic diseases. Fever, it is well

known, is highly contagious, or easily propagated from one individual to another; while all experience shows that cholera is rarely, if ever, contagious; consequently, the separation of the sick from the healthy,—a measure so essential in checking the spread of fever,—is not required in cholera; and the friends and relatives of persons attacked with cholera may be under no apprehension of catching the disease, and need not be deterred from affording to the sick, in their own dwellings, every needful assistance and attention. The Commissioners of Health, after mature consideration, do not advise that cholera should be met by an extended system of hospital accommodation, such as is needful in epidemics of fever; but recommend, in preference, a general system of prompt and efficient dispensary relief. The non-contagious character of cholera fortunately removes all objection to the receiving of persons suffering under the disease into the ordinary hospitals of the country, whether infirmaries or fever hospitals,—all of which the Commissioners of Health advise should be open and in readiness to receive destitute persons in cholera."

In the circular issued by the Royal College of Physicians in London, the following paragraph occurs: "Cholera appears to have been very rarely communicated by personal intercourse; and all attempts to stay its progress by cordons or quarantine have failed. From these circumstances the Committee, without expressing any positive opinion with respect to its contagious or non-contagious nature, agree in drawing this practical conclusion: that in a district where cholera prevails, no appreciable increase of danger is incurred by ministering to persons affected with it, and no safety afforded to the community by the isolation of the sick."

I may here remark, that the Committee of the Royal College of Physicians in London have left themselves open to the charge of inconsistency; for, while they do not express any positive opinion with respect to the contagious or non-conta-

gious nature of cholera, they recommend measures which can only be justified by *a very positive opinion indeed, that the disease is not contagious.* It was unwise to preface the practical adoption of an opinion by an expressed doubt of its soundness.

Thus I find arrayed against my views two bodies, to whose authority the public must attach great importance. But, being persuaded that I am upholding the cause of truth against error, I am by no means discouraged, and feel myself urged by a sense of duty to persevere. Neither have I been, since the publication of my article in the last Number of this Journal, entirely unsupported; for I have received communications from many eminent physicians, both in Ireland and England, approving the course I have taken; and some periodicals, both medical and literary, have expressed a similar opinion. Thus in the Medical Gazette, of the 4th November last, the reviewer of my work on Clinical Medicine thus expresses himself: "Doctor Graves believes, in common with most professional men, that cholera is an imported disease. His description of its origin and progress is one of the best summaries of the history of this pestilence which we have seen: and the facts which he has adduced of its obviously contagious character are only confirmatory of what has been already published by Copland, Watson, and other eminent observers." The reviewer then proceeds to enumerate the chief arguments I have used, and concludes by declaring that the Medical Gazette had long advocated the opinion that cholera is contagious. On one point only I wish to set the reviewer right; the passages he quotes from the chapter on cholera in the second edition of my book, are all taken from a paper printed in this Journal in the year 1840, and consequently long before either Dr. Copland or Dr. Watson had written on this question. The London Literary Gazette of the 28th October, writing upon the same subject, remarks: "But still, as cholera does exist, and becomes more pestilent, it is good to hear what Dr. Graves states about it. Well, he seems to confirm a suspicion we ventured to intimate in our last

Gazette (in spite of all the non-contagionist theories about it), that the disease might be contagious; and we instanced its propagation into the town of Hull by a vessel from Hamburgh in support of this probability."

In a series of papers signed "Medicus Galvinensis," an able writer in the Dublin Medical Press has strenously insisted upon the contagiousness of cholera, and has brought many important facts to bear upon the subject. As, however, the general expression of the metropolitan newspapers is adverse, and as the public mind must be much influenced by the specious arguments which their talented writers artfully decorate with eloquence, I am impelled by the danger which must arise from the propagation of error, again to raise my voice, and call on the Government, before it is too late, to make the necessary preparations for encountering this disease, when the epidemic reaches our shores. In a conversation which I had with a friend in the beginning of last September, I announced to him that this *vexata quæstio* was approaching a rapid solution, for cholera being then established at Hamburgh, the mode of its propagation would soon become evident. I reasoned that, if the disease extends itself by means of an atmospheric influence, it would arrive on every part of the east coasts of England simultaneously, or nearly so, and that consequently I should expect that, in the course of a few weeks from that date, the cholera would appear nearly at the same time in all the ports of England equally distant from Hamburgh; but that if the propagation of the disease depends on contagion, then we might look for its appearance only in those ports which had a direct communication with Hamburgh. The subsequent progress of events has afforded a most startling verification of this position, for, as I observed in a short letter in the last Number of this Journal, the disease has broken out in Leith, Sunderland, Hull, and London, all seaports communicating constantly with Hamburgh, while none of the intermediate towns between Leith and London have been visited, and for the simple

reason that there is no direct intercourse between them and Hamburgh. Facts such as these have made a serious impression on my mind, and have corroborated the opinion I formerly ventured to express, that the official authorities have, to say the least, acted unwisely in advising that cholera patients should be admitted into general hospitals, in place of preparing (as was done during the former cholera epidemic) special hospitals for the treatment of cholera. I asserted (and further reflection has induced me to repeat that assertion) that the mode of proceeding proposed by the Board of Health, that of admitting cholera patients into general hospitals, is likely to produce much injury, not only by endangering the lives of the poor patients labouring under ordinary diseases, but by alarming society at large, by collecting cholera patients in so many and different hospitals. During the former epidemic, the moment a poor patient was attacked with cholera, notice was sent to the proper quarter, and immediately a fitting convenience was provided, and the sick person was brought to a cholera hospital without delay. Thus, not only was a focus of contagion removed from the family where the sickness occurred, but the patient had the advantages of all those appliances which skill, combined with a liberal expenditure, could supply; and to the activity with which this system was pursued we must attribute the comparatively small mortality from cholera that occurred in this metropolis. In the name, therefore, of humanity, I implore the Government to pause before it puts into operation a course of proceeding which may be most pernicious in its results; and I call on the authorities to be guided by truth rather than by the false maxim, that it will be conducive to the tranquillity of the public mind to proceed on the principle that the disease is not contagious. But, alas! unconsciously I find myself falling into a serious error. I have recommended *truth*, and by my reasoning have endeavoured to uphold it; but what chance can there be of truth prevailing or of error being dissipated, when the great luminary which enlightens the mind of England, and serves as a

beacon to warn us of coming dangers, so far presumes upon the ignorance and credulity of its readers as to dare to print the following puerilities(*a*)? The *Times* of October 17th, in its leading article, thus proceeds: "The disorder has chiefly appeared at Woolwich, but not in Woolwich generally; nor in the river generally; nor in the convict ships generally, though they all lie so close together; nor in the Justitia convict-ship generally; nor in its lower deck generally; but on the starboard side and stern thereof, particularly and exclusively, that part being exactly opposite the mouth of a sewer which empties itself into the Thames."

I must confess that it rather puzzles me to make out how the "*starboard side and the stern thereof*" can be called *that part*, seeing that they form two very different parts of a ship; neither can I understand how these two different parts, one of which is at right angles with the other, can be *exactly* opposite the mouth of *a* sewer; but waiving this inaccuracy of language on the plea that the Thunderer, as when erst enamoured of *Europa*, still seeks gratification by making himself a—Bull,—it appears, according to this new philosophy, that a malign atmospheric influence arrived in the Thames, but of so thin, curved, and laminated a nature, that it touched not any of the numerous ships that lie *so close together*, but, winding itself amongst them and leaving them unscathed, it poured itself into this unfortunate sewer, from which it issued again forthwith, directed against the starboard side and the stern of the fated vessel. In ordinary cases, when a malarious wind reaches an opposing obstacle, such as the side of a ship, and enters through windows or port-holes, those who have berths at that side are evidently more protected from the direct influence of the vapour, than those who sleep at the opposite side;

(*a*) The greater part of the following observations on the Justitia and the theory of the *Times*, were published in the *Freeman's Journal* of the 29th October, and consequently a week before some judicious remarks on the same subject appeared in the London Medical Gazette.

and, therefore, even supposing that the air was poured into the ship like a fluid, and only capable of reaching those who were in its direct course, it is quite evident that the persons occupying berths on the larboard side should have been chiefly affected, and not those on the starboard. But the atmosphere, (and it is the only medium by which the deleterious influence could be conveyed from the sewer) is unfortunately elastic, and whether it enters the hold of the ship by the starboard windows, or by the larboard, or by the poop, it immediately diffuses itself equally through the whole internal space; and besides, I beg leave to remind the writer in the *Times*, that the inmates of the ship, convicts though they be, do not remain like statues in a gallery, fixedly arranged at different sides opposite to each other.

But in fact the subsequent course of events has completely overthrown the hypothesis of the *Times*, and has altogether exculpated the accused sewer from the guilt of having generated spasmodic cholera; for the hulk Justitia having been removed from the suspected locality, it was at last found necessary, on account of the continued spread of the disease, to remove the convicts, but not until twelve had died, and thirty-seven sickened: and then we find suspicion transferred to the hulk itself, and forthwith it was ordered to be broken up! I have no returns which enable me to state the precise number of convicts who afterwards were attacked on board the Unitè(*a*); but the number was considerable; neither can I say how many took the disease in the Hebe and Sulphur receiving ships, to which they had been transferred from the Justitia; but in the Dreadnought hospital ship several died. I should like to know whether a pernicious sewer was opposite to each of these ships; but I rather suspect that even the sewers had now fallen into disrepute with the authorities, for in the

(*a*) Five actually died in three days, from 7th to 11th October.

official returns of October, 30, we find it stated: "Almost all the cases of deaths, and even attacks amongst the convicts, have occurred amongst those who have been subjected to punishments, or confinements in the black-hole."

After this announcement we must of course suppose, that all such punishments were for the time abolished; and then indeed, what between a liberal allowance of anti-choleric tobacco, and a freedom from choleric punishment, it might be well said, "O felices *convicti* sua si bona norint!"

It gives me great pleasure to find that my views are fully confirmed by Mr. E. O. Spooner, of Bradford, in a paper read before the Dorchester Medical Society, on the 12th October, last, and published in the Provincial Medical and Surgical Journal of the 15th November. Mr. Spooner has touched on many of the subjects I treated of in my last paper, and his manner of considering the different questions I there argued is so identically the same as mine, that, had not his paper been read before mine was printed, it might have been suspected that he had borrowed largely from it. This is to me most satisfactory, and encourages me in the belief that my conclusions were founded on truth. Mr. Spooner says, that "We are not able to discover that any new facts have arisen respecting cholera, either with regard to its course, progress, character, or treatment. All the symptoms of the disease, when it has attacked any person, have been similar, and it advances in the same lines of commerce and human intercourse, taking about the same period to travel, in spite of opposing winds and every variety of atmospheric condition. The only difference has been in a somewhat more rapid march from place to place, correspondingly with the increased facilities of locomotion, and, in some degree, perhaps, to be accounted for from the abolition of all attempt at forming a '*cordon sanitaire.*'"

I cannot conclude my remarks on this part of the subject without quoting what the Editor of the London Medical

Gazette(*a*) has written on it, and I beg to direct the reader's attention particularly to the fact connected with the period of the incubation of cholera; for it is clear that, had the female in question gone to an inland town and there sickened, the case might, and most probably would have been cited as a proof of the spontaneous evolution of cholera.

" Another fact worthy of remark is, that the cholera of the Justitia has not manifested any epidemic tendency. It has been confined to this convict ship alone. The latest reports inform us that the disease has not shown itself among convicts in other ships, or among the inhabitants of the town; and we are now quite at a loss to understand how the cholera poison from this Kentish sewer has been especially conveyed to the stern of the Justitia. If there has really been a local origin of this poison on the banks of the Thames, it remains to be explained how it is that the cases which have yet occurred in this country have been among the crews of vessels on our northern coast. Besides, before admitting the sewerage theory, it would be desirable to know why cholera has not shown itself among individuals living near the mouth of other sewers which drain much more extensive districts.

" The only ground upon which this sudden abolition of quarantine can be justified is, that intercourse between an infected and an uninfected port cannot be prevented with that degree of strictness which is indispensably necessary to prevent the importation of a disease. We are not yet quite certain as to the period of incubation of cholera.

" One of the recent fatal cases at Hull was that of a female who arrived in the port from Hamburgh on Saturday night, apparently in good health. There was no case of cholera on board. A few hours after her arrival she was seized, and died the following day. The fact that there had been no case of cholera on board, and that all are apparently in good health,

(*a*) October 27, 1848.

is, therefore, not necessarily a criterion for safety. It appears to us that the Lords of the Privy Council have been somewhat premature in ordering the entire abolition of quarantine only twelve days after they had commanded its imposition. Such a sudden change in their views reflects but little credit upon their judgment; and the reason which they have assigned in their order does not justify their proceedings. They do not appear to have consulted our colleges, or to have taken any medical opinions; but they have allowed themselves to be guided partly by the views of a Board of Health, not constituted of medical men, and partly by the representations of those who are peculiarly interested in the entire abolition of quarantine. Admitting that we have the disease among us, its attacks have been limited hitherto to a few persons, and confined to a few localities. The Lords of the Council have resolved by this order that no British sea-port, however healthy, shall have any sanitary protection thrown round it.

"The proper course to have pursued would have been, in our view, to have maintained a limited quarantine with respect to all vessels from infected continental ports, making it more stringent when deaths from cholera had occurred during the voyage, or when the disease had not already shown itself in the British port at which the vessel might arrive. It is possible that, even with these precautions, not one of our sea-port towns would escape the disease; but the recent order in Council goes the length of licensing its immediate importation into healthy places, and each port may thus become a channel for the sure diffusion of the disease over the whole country.

"We must reserve for another time some remarks which we had intended to make on the diffusion of the disease by articles of clothing. Quarantine appears to be tried by an unfair test in restricting the proofs of its efficiency to the power of preventing the propagation of the disease from person to person. We believe that due inquiries are rarely made respecting the clothing of those who have died from cholera, and to this

secret and unsuspected source of propagation may be probably attributed many cases in which there is a failure of proof of personal intercourse."

Were the subject not so serious, we might smile at the reasoning of the *Times* and the folly of the authorities, who imposed quarantine as soon as it was ascertained that cholera had been actually imported, and abolished it shortly after, when it appeared that the supply was scarcely sufficient to infect the community; but the question is pregnant with consequences too important to justify even a passing air of levity; and a doctrine supported by sophistry, however absurd, is calculated to lead to consequences the most disastrous, as is feelingly shown by the Editor of the *Freeman's Journal*(*a*), who thus expresses himself:

"We discuss this question not with a view to sustain this theory or that, but because we feel it to be eminently a practical question at such a moment as this, when the dread visitant may be almost at our doors. The theory about to be acted on by the authorities is, that cholera not being infectious or contagious, no special care need be taken to sever the disease from the healthy, and, therefore, no special hospital with its accompanying expenditure (*hinc illæ lachrymæ*), is required for the treatment of the poor. God help the poor! Thrust the cholera patients into general hospitals, and you drive all other sufferers from the asylums erected for their benefit by the charitable and humane. Take the alternative,—leave them in their own miserable abodes,—and you consign them to certain death. How can the poor of Dublin be treated for cholera at their own homes? Any man who knows the social condition of Dublin, and the economy of the Dublin lodging-houses which the poor and struggling almost exclusively occupy, will not hesitate to pronounce it impossible. Several families not unfrequently occupy one room; several individuals one bed. Even where

(*a*) October 21, 1848.

a family has the exclusive use of a room, there is no "spare bed" for a patient; and if the bed be given up to the sick, all the other members of the family must sit shivering and sleeping in the cold. Judicious precaution against disease! Then where are the appliances for promoting warmth, so essential in the treatment of cholera? Blankets,—there is but one for the whole family! Hot bricks, jars of hot water,—there is no fire even to warm a drink; not even a rushlight to give the colour of warmth to the room! Yet it is here, and under such circumstances, our non-contagionist humanitarians would have the poor of Dublin treated for cholera: prepared for death would be, perhaps, the more proper phrase."

In reflecting upon the causes assigned by the non-contagionists to account for the spread of cholera, one is struck with the singularity of the fact that sewers, streams, rivers, and damp localities are blamed as the producers of this disease; for if a case of cholera occurs, and that the non-contagionists discover anything in the locality which can be accused of unusual dampness, whether it be a marsh, an arm of the sea, a drain, or a river, they immediately exclaim, "we are satisfied; we have found the 'fons et origo mali;' behold the source whose emanations have given rise to the disease." Satisfactory as this Εὑρηκα may appear to such gentlemen, I doubt whether the discovery will be considered as conclusive by those who are in the habit of reasoning more philosophically. To such persons it will appear strange that the above enumerated sources of noxious miasmata should manufacture no vapours capable of producing disease during the hot summer months; and that during the most unhealthy months of autumn, likewise, they did not give rise to cholera, in August or September; but that in the months of October, November, and December, they should suddenly become endued with a power of manufacturing noxious vapours capable of infecting the neighbourhood. The philosophical reader will, no doubt, admire the simultaneousness with which all the moist and low situations on the east

coast of England assumed in the beginning of October their new and fatal function; and he will consider it as a phenomenon unexampled in nature, that rivers, sewers, canals, marshes, rivulets, and filthy water nuisances, sources from which exhalations so different in chemical composition are usually evolved;—he will admire, I say, the unique law which derives, from causes so various, effects in every case precisely similar, viz., the production of a new specific disease. These who are satisfied with the maxim so generally adopted by mankind, "credo quia impossibile," have here abundant occasion for their faith; but see to what consequences this explanation, if persevered in, must lead us. The whole country will be despotically governed by parish authorities and sanitary committees; a new and unclassical *règime* will be formed; and its medical police, not content with the triumphs gained by flushing sewers, white-washing houses, abating nuisances, emptying dust-holes, and performing other *hyper-Augean* labours, will persecute water wherever they find it. Adieu then to the gods(*a*) who preside over fountains, over streams, over rivers; banished be the nymphs from our suburban villas; let nothing be thought of but thorough drainage, and let everything be superseded in the way of ornamental water, whether lake, cascade, or pond. It may be questioned, I say, when we have succeeded in extinguishing the chief beauties of our landscape, whether we shall have effectually exorcised the demon. May not the unclean spirit of cholera be one that "walketh through dry places"?—has it not dwelt on mountains, traversed the arid desert, and decimated the inhabitants of countries where water is not merely the chiefest but the dearest necessary of life?

(*a*) This reminds me of a strange error committed by a writer in an interesting article on Greece, published a few years ago in the Quarterly Review. He says it is very remarkable that the modern Greeks have ceased to use the word ὑδωρ, and have borrowed from the Turks the word νερον. So then our old favourites the *nereids* of Hesiod were christened by the Turks after all.

I feel by no means disposed to perform the operation of vivisection on a very active and well-meaning body of sanitary volunteers in this city; nor am I inclined to expose the utter futility of their attempts to keep the cholera out of Dublin by enforcing cleanliness. Neither do I believe that the means they possess will ever effect more than unkennelling half-buried and half-decayed odours. In the attainment of such an object, half measures render nuisances more offensive, and it is better, in many cases, to let them lie dormant than to stir them up into fresh activity. This principle was well known to Don Quixote, and was exemplified in the advice he gave to Sancho Panza on a certain occasion, to which it is now more seemly to refer than to describe. But it appears that these gentlemen have sought and obtained the assistance of the authorities, and consequently they have at once become formidable to their fellow-citizens, for they are now empowered to forward their measures by Act of Parliament; and in pursuance of the powers thus discreetly granted, I perceive that a meeting has been this day (14th November) summoned for the parish of St. Mark, in order to levy a cess for sanitary purposes. If such tax is only to be levied off the rich, it will, in the parish of St. Mark, amount to almost nothing; if it is to be wrung from the limited resources of the thousand pauper roomkeepers who inhabit that quarter of the city, then indeed will they have reason to curse the day that the means for purchasing food were diminished by contributions intended to remove nuisances, and in vain will they be told that they must pinch their stomachs for the general purposes of cleanliness. The sanitary committee may, by the force of perseverance and of the law, effect many of the objects they have in view; but I fear, even if they entirely succeed, they will have but converted the wretched dwellings of the miserable inhabitants into white-washed sepulchres. Had funds been raised sufficient to feed, clothe, warm, and lodge the poor inhabitants, had hospitals been provided for the reception of cholera pa-

tients, had means of carrying them to these hospitals the moment they were attacked been ready; then indeed, when the enemy arrived, some effective resistance might have been made, and if the plague could not have been averted, it might at length have been stayed. But, alas! nothing of the sort has been done; the public have been misled by the Government, and it is doubtful to decide whether the Government have been misled by or have misled their medical advisers, but in either case the poor will be the sufferers. If they are hungry it would be in vain to tell them that some neighbouring alley has been cleansed, and some adjacent sewer flushed. Hungry, chilled, and crowded together, they present meet fuel for the epidemic; and the more rational of them, when exhorted to be cleanly, will answer like the hungry negro, who, having patiently listened to a sermon addressed to him by a zealous missionary, and intended to convert him, submissively answered, placing his hand upon his empty stomach: "Massa, belly sick, bad palaver."

One great discovery has been made by the Board of Health in London, and which is announced in the following words(*a*): "The places in which the pestilence is now numbering its first victims are the very spots which are known to be the filthiest in their respective districts, and to be the constant seats of typhus fever and other epidemic diseases. In tracing the individual cases reported to the Board of Health, the medical inquirers who, under the direction of the Board, have made a special investigation of the circumstances connected with the earliest attacks of the disease during its present visitation, have been led not only to the streets, courts, and alleys, but sometimes even to the very houses, *that are notorious as fever nests.*" So then it appears that the *fever nests* have mysteriously and unaccountably become cholera nests, that the same raw materials have been used for the production of a to-

(*a*) *Morning Chronicle,* November 3.

tally new manufacture, and that a new effect has been created by an old cause. But were not the localities indicated by the Commissioners nests not merely of fever, but of all diseases? Had not, during all seasons and in every year, scarlatina, measles, hooping-cough, and small-pox, hatched in such places? Are not these localities filled with the victims of indigestion, marasmus, scrofula, and itch? Are all these evils produced by the incubation of the same ova? If so, then Pandora's box has re-appeared in a new and more frightful form, even *Hope* is scared from its bottom, and all the evils to which flesh is heir may be caused by a superabundance of hygrometric moisture in the atmosphere(*a*). Upon the dampness of localities, and the wetness of the weather, the genius of observers has chiefly been exercised, as is exemplified in Dr. Adair Crawford's report upon the outbreak of cholera in Petersburgh in the present year; but do these gentlemen forget that there are places in Hindostan, where more than 200 inches of rain fall annually; that, during many months of the rainy season on the west coast of Africa, it is a constant deluge, everything flooded, the air so charged with moisture that it is impossible to keep anything dry, that the best leather decays, and the most carefully painted iron rusts? Do they forget that, in the great rivers of South and North America, the whole country, to the extent of forty or fifty miles on either side of the rivers, is flooded, and the civilized inhabitants obliged to reside in the tops of their houses, while their more savage brethren betake themselves to their nests constructed amidst the branches of trees? Do they forget the vast population which resides in the boats innumerable that cover the

(*a*) November 13th.—I see that a correspondent of an English paper, writing from Berlin, under the date of November 6, says: "Cholera is fast disappearing in this city, although for the last ten days much rain has fallen, and the weather has been unusually damp." It is plain that this correspondent was neither employed by government, nor yet was he writing for the benefit of British capitalists.

surface of the great Chinese rivers? And yet all these physical causes, believed by these gentlemen to be capable of giving rise to cholera, have existed for ages, and have been powerless to generate this fell destroyer. The advocates for the production of cholera in England by means of the operation of physical causes asserted that in 1832 the actual outbreak of the disease was preceded by a certain change in the air, not sufficiently intense to produce actual cholera, but which gave rise to indigestion and various degrees of intestinal inconvenience, and even diarrhœa. This state of the air they assumed as a necessary precursor of the more noxious atmospheric changes that were soon to follow; and, knowing that their hypothesis would appear more credible if they persuaded the public that it had actually preceded the cholera, they at once set about manufacturing facts, and produced such a goodly array of assertions respecting the invariable prevalence of bowel complaints for weeks before the invasion of cholera, that their opponents were unable to answer their arguments without questioning their veracity, and thus the non-contagionists were necessarily triumphant, at least upon this part of the question at issue. But, alas! the accurate *data* which the Reports of the Registrar-General now furnish have entirely dissipated this illusion, for it is now proved that the public health was never in a more satisfactory state than it had been for some weeks before the outbreak of cholera! The atmospheric change, the passage from wholesome to noxious, must have been, therefore, so sharp, so well-defined and sudden, as to be *incredible*,—I say *incredible* advisedly, because it must be borne in mind that for many weeks *after the cholera had gained a footing* in Edinburgh and London in 1848, these reports prove that in all other respects the public health continued good. Is it then credible that the atmosphere could be thus interstitially changed, so as to be in its general mass more than usually wholesome, while it contained here and there isolated portions of deleterious air?

So confident do the non-contagionists appear in the truth

of the theory they advocate, that they scruple not (and here I must admit their honest candour) to introduce facts which evidently militate against their own opinion. Thus, in a circular printed by the Metropolitan Sanitary Commission of London, in the evidence of Samuel Rogers, Esq., Fellow of the Royal College of Surgeons, page 5, he states: "I shall conclude my remarks by noticing an idea prevalent in India, and as appears not without foundation, that cholera attaches itself to large bodies of men in camps, and is by them conveyed from town to town, if it is not in some instances generated by large assemblages of people. In 1831 the 45th regiment of Native Infantry, while on their march from Madras to Kolopore, was attacked with cholera, which moved along with them, infecting every village they passed through. They reached Ballary on the 1st of February, the disease not having been seen there for many months; on the 2nd several cases occurred in that place, and from this period ten or twelve deaths were reported daily. Cholera is a usual attendant at native festivals, where crowds of people are collected. At Juggernaut it is an annual visitant. The town of Pooree contains 35,000 inhabitants, and the number of pilgrims sometimes amounts to 150,000. The inhabitants are usually quite healthy before the occurrence of the festival, which takes place in June or July; but immediately on the arrival of the pilgrims, and when the lodging-houses are literally crammed with inmates, cholera suddenly breaks out, and in the course of a few days hundreds are cut off by it. This is not an occasional or accidental occurrence, it is an invariable one; and the disease which has been thus generated as suddenly disappears on the dispersion of the crowd, a few isolated cases only occurring for two or three days after the departure of the pilgrims." Now, in my humble opinion, these facts inflict a heavy blow and great discouragement on the non-contagionists, and it was evidently felt by the committee who were examining Mr. Rogers, that he volunteered to say in his concluding re-

marks what they would have preferred to have remained unsaid, for immediately some member of the committee took him to task, and endeavoured to elicit, by subsequent queries, answers which might help them to hobble out of the difficulty into which they had been unexpectedly plunged.

Neither time nor space permits me to insert the queries put, or the answers elicited; but the information thus gained could not possibly shake the conviction to which the above facts, stated by Mr. Rogers, must inevitably lead every impartial inquirer. It might and did happen that a regiment affected with cholera, and leaving the station at which the disease at first appeared, would bring the seeds of disease with them; or, to express the opinion of the non-contagionists more clearly,—a body of men that in a station had inhaled the cholera miasmata, might not exhibit any symptoms of the malady for several days, during which time they would have proceeded far upon their march. This explanation is quite admissible, and may be received, so far as regards the illness of the soldiers, in spite of their change of place; but it altogether fails to elucidate how the natives of the villages, where no cholera existed previously, were attacked with the disease immediately after the arrival of the soldiers.

With respect to the hypothesis, that the cholera was generated among the pilgrims or the inhabitants of the city, from the mere fact of its being *over-crowded* during the sojourn of the pilgrims,—it is too absurd to justify a serious examination; while, on the other hand, all the circumstances of the case are quite consistent with the doctrine of contagion.

It may deserve consideration how far we can account for the cessation of cholera in towns, often long before the whole body of the inhabitants are affected. The truth seems to be, that a great number of constitutions are not susceptible of the disease, or have it so lightly that the individuals are scarcely conscious of its presence. This position is warranted by the fact that during the prevalence of cholera, *as a*

widely spread epidemic, in any city, many people are found complaining of flatulence, slight nausea, diminished appetite, borborygmi, freeness of bowels, and other evident symptoms of gastric derangement: such persons, in my opinion, labour under the infection of cholera, and such cases are analogous to what we so frequently observe in persons exposed to the contagion of measles, scarlatina, hooping-cough, and even small-pox, and in whom the effect produced by the poison from each of these diseases is so slight that they are but slightly troubled, and they are not obliged to desist from their usual avocations. When, then, in any society or city, a certain number of persons have been gravely attacked with cholera, and the great majority have had it almost without knowing it, then the disease ceases from want of individuals it can affect. What the circumstances are which render the inhabitants of some cities liable to be struck with a heavier form of the disease in a greater proportion than the inhabitants of others, we probably shall never be able to explain; but many instances might be brought forward, where in two neighbouring cities the population of the one was nearly destroyed, and the population of the other comparatively spared. That such an occurrence is observed in other manifestly contagious diseases, is evident from the answers which I received to queries addressed to medical men practising in the various towns of Ireland(*a*), and from which answers it appears that scarlatina has for many years caused a great loss of life in some towns or villages, while it was either mild or even almost unknown in contiguous localities; a fact exactly analogous to that which has excited so much astonishment with respect to cholera, and which, however unaccountable, is, therefore, not without parallel.

In confirmation of the conclusions derivable from these answers respecting scarlet fever in Ireland, a fact mentioned

(*a*) See Clinical Medicine, second edition, vol. i. p. 324.

by Mr. Spooner in the Provincial Medical and Surgical Journal, already referred to, deserves special attention, for he states "that small-pox and scarlet fever frequently rage in Exeter, without extending to the adjacent villages, Topshaw and Dawlish. Will any one doubt the contagious nature of these diseases on that account?" adds Mr. Spooner. And he very justly considers this as quite analogous to what Dr. Shapter considered to be a very curious circumstance with respect to cholera, viz., "that the two first cases which occurred in Exeter came from two infected places, London and Plymouth, after which it spread to such an extent that 400 of the population died;" and yet he says that none of the adjacent villages were visited by the epidemic; Topshaw, Dawlish, and Crediton, having only a case or two, which did not extend further.

The great plague of London of 1665 supplies us with a very remarkable example of this apparent anomaly in the course of a disease confessedly contagious; for it appears, that of the 130 parishes in and about London, four parishes entirely escaped the infection, and some other parishes were free for months, while others were depopulated(*a*). Here, then, is an omission, on the part of the plague, of places, quite as remarkable as any of the *skipping motions* which authors have recorded with so much wonder in the case of cholera. We now see that such anomalies in its progress are not sufficient to take it out of the category of infectious diseases.

A very striking example of the escape of certain localities from the infection of cholera is mentioned by the intelligent Roman correspondent of the *Daily News* of the 16th November last, viz., that during the last epidemic of cholera at Rome, the "Ghetto," a part of Rome inhabited by the Jews alone, altogether escaped. Now it is perfectly well known to all those who have visited the Eternal City, that this very quarter is re-

(*a*) See Vincent's "God's Terrible Voice in the City," printed in 1667, and other authentic accounts of the plague of 1665.

markable for a concentration of every nuisance, whether solid, fluid, or gaseous, that is capable of exciting disgust or causing disease. If, then, unflushed sewers, stagnant cess-pools, noisome dunghills, and all the countless abominations which render the quarter inhabited by the descendants of Abraham(*a*) a bye-word even among the Romans,—themselves the filthiest of Christians,—if, I say, cholera spared the Jews inhabiting such a locality, can cholera be produced anywhere by those agencies, on which the advocates of dirt and want of drainage as its causes, so confidently rely?

The progress of cholera in England, in 1832 and 1848, exhibits the extreme difficulty with which it gained ground, a difficulty exactly such as a contagious disease might be expected to exhibit in a country whose inhabitants live in separate families and in distinct houses, and where the fear of contagion most effectually prevents persons from holding, at least willingly, any communication with infected individuals. In consequence of these difficulties, cholera has to struggle long before it gains admission into Great Britain; and several months may be reckoned on as the duration of the struggle both in 1832 and 1848. In Leith and Edinburgh cholera spread in both cases with greater rapidity than in Hull, Sunderland, and London, and for the simple reason that in Scotch towns the houses, especially in Edinburgh, are constructed after the continental fashion, and each residence includes many families, who gain admission to the different *flats* by a common staircase; these circumstances facilitate the propagation of contagious diseases; and the more rapid diffusion of cholera in Edinburgh than in London is, therefore, an additional proof of its infectious nature. With reference to this explanation of the more rapid diffusion of cholera in Edinburgh than in London, I may remark, that my

(*a*) Who is to blame for this wretchedness of the Jews in Rome, Prague, and other continental cities? Certainly not the Jews themselves. They are "cabin'd, cribb'd, confin'd" sorely against their will; and Christendom cannot be called civilized so long as *they* are kept in such a state of thraldom.

views are borne out by some observations of Mr. Spooner's, who says, "That in England the general immunity from cholera, at the period of its last inundation, arose probably from the fear of contagion inducing strict measures of separation and cleanliness, as well as from the comparatively small population of our towns, and the scattered character of our villages, and the *cordon sanitaire* of our cultivated fields and belts of downs." This quite agrees with my hypothesis; for I am persuaded that it is the comparatively complete isolation of English families, whether poor or rich, that renders the inhabitants of England less liable to depopulation from contagious epidemics than continental countries; and consequently the rational physician and the prudent legislator should avail themselves of this advantageous condition of the English population, and should have recourse to such measures as will tend still further to check the progress of disease. I quite agree with Mr. Spooner, that the avowal of the truth, either with regard to cholera or the plague itself, is not likely to be productive of mischief, but, on the contrary, to induce wholesome measures of caution, such as have been before employed with success in preventing the spread of cholera. Thus he says: "In the little village of Bere, in this county (Norfolk), there were, in 1832, twelve cases of genuine Asiatic cholera, of which six proved fatal. The first case was of a person who came from London. Great care was taken to separate the sick from the healthy, and the disease did not spread to any adjacent village. A few cases of cholera occurred at Pool and at Bridport in this county," he adds, "but strict attention to cleanliness, ventilation, and those cautions suggested by a general belief in its contagious properties, contributed very much to arrest the progress of the disease." I am quite aware that many persons are alarmed at the notion that cholera is contagious, and think that society is threatened with a greater evil, if this statement is true; this view of the subject can easily be shown to be fallacious, for if cholera be a disease

depending on general atmospheric influence, its propagation cannot at all be guarded against, and the exertions of man will be of no more efficacy in impeding its progress than they would be in arresting the march of influenza: but if the disease spreads by contagion, human means may manifestly, on many occasions, be brought into play with benefit. This view of the subject ought, therefore, to prove more consoling to the mind than the other.

The length of time which cholera takes to make its ground good in England, amounting, as I have shown, to at least a few months, establishes the difference for which I before insisted between the influences that gives rise to the propagation of cholera and of influenza. Let us take as an example the influenza of 1847 (an account of which has been published by Dr. Peacock), and it appears that the epidemic in London was most active from the 22nd to the 30th November, having commenced about the 16th or 18th of that month, and ceased to be very prevalent between the 6th and 8th of December. The influenza, therefore, arrived at its maximum in about a week from its first appearance, and, continuing to rage with intensity for about eight days, then declined so rapidly that in ten days more very few were afflicted. In a work upon cholera published by Dr. Gavin Milroy, he writes much upon the analogy that exists between the spreading of cholera and of influenza, in order to prove that the former, like the latter, depends on the atmosphere for its propagation; but, in my humble opinion, he has been altogether unsuccessful in his attempt, nor do the facts which he has rather ingeniously brought forward justify me in altering the opinion I have already expressed in my Clinical Medicine, that the spread of influenza is strikingly different from that of cholera. We have seen already how much more suddenly the former arrives at its maximum; let us now examine the rate at which the influenza of 1847 travelled, and compare it with the rate at which cholera progresses.

The epidemic was raging at Constantinople in August, and it affected the south of France and the shores of the Mediterranean in October. In Paris it commenced about November 27, and was at its height on December 4. In Madrid it was very general on the 11th and 19th of January; and in Geneva it appeared as a casual disease in the first week in December, suddenly became very prevalent about the 20th, declined during the course of January, and had almost disappeared by the commencement of February. Observe what an extensive zone is occupied or overshadowed by the atmospheric influence producing influenza, and how rapidly that shadow overspreads all the country round and included between Constantinople, Geneva, Paris, Madrid, and London, an immense tract being thus occupied by the disease in less than three months. How different this from the slow progress of cholera! Again, how evidently do these two diseases differ as to the proportion of persons attacked by them:—thus in Christ's Hospital, Dr. Peacock relates, that of the 1000 boys, 350, or about one-third, were more or less affected by the disease in three weeks; in Paris, between one-fourth and one-third of the population is said to have suffered from the influenza in 1847; and in Geneva the proportion affected is said to be not less than one-third; while the epidemic influenza of 1837 affected fully one-half the inhabitants of London and of Paris. Here again, the difference between cholera and influenza, is so striking as to require no further elucidation.

The Board of Health, both in Dublin and in London, and the College of Physicians in the latter city, here it appears to me, incurred an immense responsibility; for they have asserted that nursetenders and others, whose duty brings them into immediate contact with cholera patients, are not more liable to contract the disease than individuals who never approach such patients. It will be seen just now that this assertion is altogether unfounded; but, in the mean time, I may

observe that, granting that in well-regulated hospitals, or in well-ventilated houses, the nursetenders and physicians generally escape, this frequent immunity on their part is no proof of the non-contagiousness of the disease; for, as Medicus Galvinensis, in the Dublin Medical Press of the 1st November, 1848, very sensibly remarks, "while admitting all this, I ask, is not the same true *of all other confessedly infectious diseases?* In how many instances, during the late epidemic fever, have I in vain endeavoured to trace infection? How often, in the prosecution of my dispensary labours, in remote and isolated hovels, have I been assured in such and such a case, such a cause could not have operated; and do I, or does any man, doubt the infection of typhus? How often, in private practice, when I knew the habits of a family, or could depend upon their co-operation, have I, upon the occurrence of fever, calmed their minds, by stating confidently that, with the most ordinary precautions, the disease would not spread; and, even in protracted attendances upon the worst forms of fever, this confidence has hardly, in a single instance, been disappointed; and yet, upon this weak argument of the immunity of medical and hospital attendants, how many anti-infectionists ground their belief!"

Nothing is more common than to find cases of measles, scarlatina, or hooping-cough, occurring in families living in the country, and in whose neighbourhood we cannot discover that any other similar cases exist; in all such instances, however, no one ever dreams of attributing the origin of these diseases to the atmosphere, or to local *miasmata;* on the contrary, every rational physician explains the occurrence on the supposition that such cases originated in infection, although he has been unable to discover its source in this particular instance. The hypothesis of the contagiousness of cholera, by no means, therefore, presupposes the possibility of our being able always to point out the sources of infection in each case.

My friend Dr. Little, of Sligo, writes to me upon this

subject: "I care not for your proofs of negation, let them be ever so numerous. I have had three of my own children in the nursery with three other of my children in the hooping-cough, and yet they never took it. I have had fifty cases of typhus fever this season, in houses with large families, and no second person in any of these houses took fever. Am I then, from these facts, to conclude that neither of these diseases is contagious? I care not, I say, for fifty negative facts; give me one positive fact, it is worth a thousand of them; and if cholera *is rarely, if ever, contagious,* I say it is madness not to act as though we believed it to be universally so; and yet the Board of Health, both here and in London, force us to act as if the disease never was contagious, and they assure us that no one will incur any danger by this mode of proceeding."

The following paragraph, taken from an article in the *London Evening Mail*, and published some time towards the middle of last November, contains so complete and well-digested a *resumè* of the chief errors I condemn, that its preservation seems desirable, if it were only for the purpose of enabling our successors to form a just estimate of the sanitary theories now promulgated *by authority*. In speaking of the new notification of the London Board of Health, the writer in the *Mail* observes:

"It will be seen from the important documents which we this day publish, that the highest authorities of the country agree in calling the cholera *epidemic* rather than contagious. They very wisely do not commit themselves either to the contagion or the non-contagion theory, confining themselves to a practical view of the disease. *Experience* proves that there is no perceptible danger in waiting upon cholera patients, and that whoever runs away from them is, in fact, avoiding an imaginary peril. The touch, the breath, the smell, or whatever has to do with the person of the patient, is as innocuous as the mere spectacle of suffering. To all intents and purposes, therefore, there is no contagion of the disease. But that same expe-

rience indicates certain almost uniform antecedents of cholera. Whatever has usually caused a dispositison to typhus, to scarlet fever, and other epidemic diseases, will now produce cholera. The inhabitants of the same house, or court, or district, are affected by the same injurious circumstances. It is not the breath of one another that affects them, but the air they all breathe, the common miasma of the dunghill, the cesspool, the sewer, or the Thames. Certainly, as one passes along even the more open streets of the city, one may perceive how superfluous it is to suppose a more lethal effluvia than that which a whole neigbourhood is often forced to breathe. The mouth or the hand of the cholera patient cannot be more deadly than the fetor which the grate vomits forth night and day, or which stagnates over the unspeakable abominations of 'the court within a court.' Meanwhile there is an immense practical difference between the words 'contagious' and 'epidemic.' The former blinds people to the facts of the case, and deters them from its duties; the latter directs attention to the most important facts, and enforces the duties by the additional stimulus of self-preservation."

The wisdom for which this writer in the *Evening Mail* gives the Board of Health credit seems to me to be of a very questionable nature; for he praises the Board for not committing themselves to the contagion or non-contagion theory, but for confining themselves to a practical view of the disease. Now it seems evident that the Board of Health manifestly committed themselves to the non-contagion theory, for all the directions they have given on the subject proceed on the principle that the truth of this theory has been demonstrated. We find also that the writer states, "that whatever usually causes a disposition to typhus fever, scarlet fever, and other epidemic diseases, will now produce cholera." This is, assuredly, most miraculous, that any given emanation from a sewer, or any other given exhalation from damp ground, can at any one time produce typhus fever, at another scarlatina, at another (for all

these come under epidemic diseases) small-pox, or measles, or hooping-cough, or influenza. I find this new theory, industriously supported by the Board of Health and the daily Press, has found adherents in the profession. Thus, Dr. Ogier Ward, in a debate at the Westminster Medical Society, November 4, and which is reported in the Medical Gazette of November 17, " referred to scarlet fever as epidemic in Kensington and Fulham; he traced its prevalence to bad drainage; on improving this, the disease was mitigated, and then disappeared." If Dr. Ward's method of removing scarlatina succeeds, there is no end to the benefits to society that must result; for, under a complete system of drainage, we may expect all the other epidemic diseases to cease. It would be well to chronicle the divers evils, to register the various species of manslaughters, any sewer " that exhales the lethal effluvia" of the writer in the *Evening Mail* commits from time to time. In January, therefore, it vomits forth scarlet fever; in February, measles; in June and July, typhus; in August, hooping-cough; in September and October, small-pox. Such has been the offensive course of these criminal sewers for several centuries. But lately, tired of the monotomy of effect hitherto produced, they have added, by way of variety to their home manufacture, a new and special disease termed cholera. Such are some of the important facts, to which the writer in the *Mail* alleges the epidemic theory will conduct us. I must confess that I am among the number blinded to the facts of the case as he teaches them. But, nevertheless, I do not feel myself discharged from the duties which my belief in its contagion inculcates. I have no doubt that the modes of preservation which are derivable from that belief would be much more efficacious and useful to society, than all the efforts of those who inculcate the necessity of universal drainage. To me it appears that the sum and substance of accurately verified facts prove nothing strange in the progress of cholera; nothing to distinguish it from the manner in which other contagious

diseases are propagated. True it is that wonders have been daily published, and miracles recorded, concerning its mode of travelling, and manner of attacking towns, peoples, and armies. But, like most modern miracles, an accurate examination strips them of all pretensions to that character, and shows that a belief in them is founded on misrepresentation and inaccuracy, frequently, perhaps, unintentional, but always blameable. I think that I have shown to the satisfaction of my readers, that the so-named vaunted peculiarities of cholera are shared in common with it by other contagious diseases; and, if I am not mistaken, I have proved that in its diffusion it obeys the same laws with them; and if this be true, it follows, as a necessary consequence, that all proceedings founded on the recommendation of the Boards of Health in London and in Dublin are at once injurious to the public health and unjust to the patients attacked, for the non-separation of patients increases the diffusion of the disease, and renders it impossible to supply to the sick the attentions they require. The injury that will accrue to the profession if the present epidemic becomes formidable is sufficiently obvious; for, as there will be no hospitals in which cholera patients can be treated, physicians will be distracted by the multiplicity of calls; they will be fatigued by going from one abode of misery to another; they will be dejected by finding everywhere a want of those appliances that are actually necessary; and finally, being worn out by an attempt to discharge duties that are beyond their strength, and utterly impracticable, they will resemble a *forlorn hope*(*a*) or-

(*a*) Military and civil writers of the present day seem quite ignorant of the true meaning of the words *forlorn hope*. The *adjective* has nothing to do with despair, nor the *substantive* with the "charmer which lingers still behind:" there was no such poetical depth in the words as originally used. Every corps marching in an enemy's country had a small body of men at the head (*haupt* or *hope*) of the advanced guard, and which was termed the *forelorn hope* (*lorn* being here but a termination similar to *ward* in *forward*), while another small body at the head of the rear-guard was called the *rearlorn hope*.—See "A Treatise of Ireland by John Dymmok," p. 32, written

dered by a bad general either to lay down their lives or perform an impossibility.

The following facts are humbly submitted to the consideration of the Dublin Board of Health, who have been so kind as to inform the profession and the public that they are "anxious to impress on all persons the important difference that exists between fever and cholera, with respect to the mode of propagation of these epidemic diseases. Fever, it is well known, is highly contagious, while all experience proves that cholera is rarely, if ever, contagious."

Although I do not exactly understand how a disease can be *rarely* contagious, still the expression is intelligible; but when the Commissioners of Health assign yet stricter limits to the extention of the word *rarely*, and say, "*rarely, if ever, contagious,*" I must confess myself utterly puzzled. Let us examine, however, how far cholera deserves to be called a disease *rarely contagious;* for the present we must leave *if ever* out of the argument, reinforced though it be by the aid of experience,—no, I beg the Commissioners' pardon, *all* experience,—that is their precise expression!!

In a letter I have just received from Dr. Simpson, of Edinburgh, he states: "In our hospital here for cholera four nurses have been already attacked, three fatally." Dr. Dillon writes me word that in Tuam four out of ten nuns, who charitably employed themselves in attending the sick in 1832, caught the disease, and two died; while three of the men who were employed to bury the dead died: and in Castlebar two out of five medical men, who attended cholera patients, died. Dr. Little, of Sligo, informs me, that of nine medical men

about the year 1600, and lately printed by the Irish Archæological Society. A reference to Johnson's Dictionary proves that civilians were misled, as early as the time of Dryden, by the mere sound of a technical military phrase, and in process of time even military men forgot the true meaning of the words. It grieves me to sap the foundations of an error to which we are indebted for Byron's beautiful line: "The full of hope, misnamed forlorn."

attending the sick in Sligo, seven died in the course of three months. These facts strongly confirm the statements sanctioned by the Board of Health in Dublin, *that all experience* proves that cholera is rarely, if ever, contagious, and that there is *no danger* to those who attend *cholera.*

Mr. Beatty, the apothecary who died in Sligo, was a Tirera man, and when he died his clothes were bundled up and sent to Tirera: the girl who took them to the river, where for safety they were sent to be washed, was attacked with cholera that night and died. Dr. Simpson states that cholera broke out very violently last week, within three miles of Edinburgh, in some paper mills. Three young girls were first attacked; they were working at a paper mill, among rags imported from the Continent, where not yet ascertained. That articles of furniture may become infected and give rise to the disease, is proved also by the following remarkable cases, which Medicus Galvinensis, whose able papers in the Dublin Medical Press I have already spoken of, has communicated in a postscript to his last paper. As I have the happiness to know the talented author who has assumed this *nom de guerre*, I can testify that every reliance can be placed on his facts.

"The following remarkable cases occurred in the practice of Dr. Nelson, of Killala, county of Mayo, by whom they have been kindly communicated to the writer:—'When cholera raged at Ballina, as elsewhere, the panic was considerable; they buried clothing, and destroyed bedding, &c., by throwing them into the river Moy. About a mile down the river a fine feather bed was seen floating by a woman, who carried it off about ten miles from Ballina, to the village of Mullinacrush. She was seized with cholera that night, and died in a few hours. Her sister in the same house sickened and died in a few hours after. Three children in the same house were all attacked, of whom one died, and the immediately adjoining houses were attacked. A female friend, who lived in an isolated cabin, visited this house, and was attacked immediately

on her return, and died. No other cases occurred in the neighbourhood. A stone-cutter employed at the Roman Catholic cathedral of Ballina, who had a house about five miles distant, at Moyne, near Killalla, had some of his children with him at Ballina; one of them sickened there and died. He then sent the rest to the mother at Moyne (three in number), these sickened and died. A journeyman who lodged in the house with this man's wife, at Moyne, took fright and fled to Killala. He took ill the day he arrived at Killala, and died. Immediately after the malady spread all over the town of Killala (his being the first case there), several cases occurring in the mean time at Moyne, all of which died refusing medical advice and assistance.'"

The following fact, bearing on the same question, rests on the same authority: "Cholera first appeared in Tuam (county of Galway) on the 4th of June, 1832, having attacked Galway, the nearest sea-port, on the 12th of May preceding. The first case was a fish kedger's wife, *who had purchased the clothes of a woman who died in the cholera hospital of Galway.* She died the following night. Her sister, from a remote part of the town, who attended her and washed her body, was the second. A relative from a neighbouring village, who also attended her, and assisted in the washing, was the third. None lived above twelve hours.

"Cholera ceased for the first time in Tuam in August; the bedding was all put up clean in store. In September, the court-house (which had been our hospital), was required for the sessions; Mr. Hartnett, the dispensary apothecary, with two other persons, were employed for nearly a whole day in the removal of the stores. *Mr. Hartnett and one of the men were seized with cholera the following night, and both died.* There had not been a case of cholera in Tuam for upwards of a month.

"Mrs. Thornton, the widow of an old horse-breaker, entered a house where Dr. Little (at present of the Sligo infirmary),

the principal medical officer of the Tuam cholera hospital, and Mr. Rothe, the Protestant curate of the town, were: they had just left the hospital, after a protracted visit. She immediately on entering expressed herself faintish and impressed with a disagreeable smell. In less than an hour she was in the full collapse of cholera. Her son, an ill-conducted drunkard, entered the house on the alarm of his mother's illness, rudely denied it, and wished to prevent her conveyance to the hospital: to show his disbelief he kissed her several times. At 2 o'clock, A. M., he was brought to hospital, where both died in a few hours. During the first irruption of the cholera we lost six nurses and four porters, having six of each.

"A few days after this a prostitute arrived in town with a detachment of military. She was seized with cholera on her arrival, and died that night. Her death was followed by three others, all within half a dozen doors of the house in which she died. Mr. Robin Potter, linen merchant of Tuam, who lived in the immediate neighbourhood where these cases occurred, was attacked with cholera a day or two after. I attended him in consultation with Dr. Bodkin, at present of that town; he lived but nine hours. During his illness I had repeatedly warned his maid-servant, who was uselessly officious in her attendance, and hung over his person, sobbing, and wiping his face, to take more care of herself; the following day she was attacked with cholera, and died in the hospital. A relative who came from a village in the country, to see her in hospital, was attacked the following day, but recovered. The man who placed this gentlemen in his coffin was attacked the same, and died the following day. Mary Qualter, the servant maid of a Mr. Egan, a solicitor, who lived in the adjoining house to No. 1, had attended her friend in No. 2, before her removal to hospital; she was seized with mild cholera the following day, and recovered."

The following facts tend to prove the correctness of the opinion published by the Board of Health, that persons ought

not to be afraid to approach cholera patients; that in truth, if there is no apprehension there is no danger. Dr. Little, says that in Sligo, in 1832, a man of the name of Middleton, who was rather an enthusiast in all he did, placarded his house and all the town thus, "fear is cholera, and cholera is fear;" and he preached about the streets on this text, warning the people against entertaining fear. This man went most zealously and fearlessly to all infected places, to prove his doctrine; but, like Maclean and Wright, who died of the plague, he caught the cholera and died a victim to his own theory.

After recounting some cases, manifestly proving the contagious nature of cholera, Dr. Little thus proceeds in a letter with which he has favoured me:

"The idea of the contagious character of cholera was forced upon me when it was scarcely a contested point, and when there was not that opposition to the opinion, which only has the effect of strengthening prejudices. In fact there was scarcely a single case which I had, out of 550, attended in Tuam in 1832, which I could not and did not palpably trace to contagion, as satisfactorily as ever such a tracing could be effected. Every case of cholera which came from any house was followed by one, two, or three more; and in many instances where patients returned from hospital, cases were brought into hospital, from the houses into which they went, within twelve or twenty-four hours of their entrance. We lost four nurses out of ten, and three porters out of nine employed died; insomuch that, of the several medical men we had attending the hospital, not one remained who entertained the slightest doubt of the contagious nature of the disease. I kept in my house the medicines which we had for the hospital, over and above those in absolute use: they lay in my study. One day, in my absence, two of the porters, though prohibited from coming to my house, were sent down for medicines. My sister-in-law, Mrs. Westropp, brought the key to them into

the hall, when she fancied that some peculiar odour arose from these men; she that night sickened, and suffered a very severe attack of the disease, being dangerously ill; the maid-servant who attended her sickened two days afterwards, and was also very ill."

As to the testimony of the French physicians respecting the contagiousness of diseases, Dr. Little, of Sligo, observes:

"My intelligent friend in Sligo, Dr. Power, who has spent much time in attending the hospitals in Paris, tells me that they indiscriminately admit or leave cases of typhus fever among the other patients; for, even in their lying-in hospitals, he frequently saw the disease extending to patients in the adjoining beds. He remonstrated with some of the most eminent men in Paris on the subject; they admitted the fact, but still continued the practice."

Surgeon Ellis, of this city, has kindly imparted the contents of a letter received from Mr. Heath, of Arklow, dated October 25, 1848, in which he says:

"I have received your's of the 24th inst., and assure you that I feel pleasure in forwarding to you any information in my power to give. As to the mode of introduction of cholera into this town, you are quite right. A fisherman named Kenny, seeking the price of oysters sold to a person living in Great Britain-street, in Dublin, was told that the party who owed him the money was lying dead of cholera in the room adjoining the shop or cellar; he waited, however, until he obtained the money; and on his passage from Dublin to Arklow was attacked with cholera, and died on the following day in his own house. The second and third cases of cholera which happened in Arklow were persons who had been in attendance on this fisherman, and his wife and child were the fifth and sixth cases. During the first outbreak the cases were nearly confined to our fishermen. The impression on my mind was and is, that the disease is decidedly contagious; but I would say not at all so in the first stage, but some few hours before

death, when I always observed a peculiar and unpleasant unctuous feel in the skin, and a fetid exhalation from the body, to which, whether rightly or not, I attributed the danger of infection."

Dr. Swan, of Kingstown, has obtained for me an account of the following case:

"A man named Sweetman, belonging to Balbriggan, died of cholera in Dublin, five or six weeks after cholera had ceased in Balbriggan. His body was brought to Balbriggan for interment. Four of his relatives, named Sweetman, carried the coffin to the church; all of these men were attacked, and two of them died. And, while the coffin was in the chapel, several children got into the hearse to play; they were attacked with cholera, and some of them died."

The following instance of the mode in which cholera first appeared in a small village, in the west of Ireland, has been furnished to me by Mr. Wilde:

"In the summer of 1832 cholera devastated most of the large towns of Connaught, particularly Sligo, Castlebar, and Galway. Acting on the idea that the disease was contagious, the magistrates and local Boards of Health instituted a sort of sanitary cordon around their respective localities, in order to isolate themselves, as much as possible, from the spread of the disease from surrounding districts; and beggars and travellers in particular, or persons coming from any of the seats of the disease, were prohibited from entering any of the small towns, or approaching a habitation in any way; and, besides the police, a description of special constables were employed to enforce these regulations.

"I was employed to attend cholera in the district of Kilmaine, in the summer of the year referred to. This village (which is on the road between Galway and Castlebar, two places then devastated by cholera), is situated on an elevated ground in the midst of an open country, principally of pasture land, and then contained about 400 inhabi-

tants; it is approached by three roads, which meet in the centre, and its streets are wider than in most villages in Ireland. For many weeks the sanitary cordon was preserved with great strictness, and no one was permitted to remain in the town, who it was believed had come from any of the infected places around (of which Ballinrobe was one), until a family removing from Galway to Castlebar arrived late at night in the village, unknown to the police or the usual watchers, and took up their residence at the house of a man named Mulvey, a shoe-maker, where they remained till morning, but departed early, fearing the police, &c. I subsequently learned that this family proceeded to Castlebar, the wife being at the time ill of cholera, where she died a few hours after her arrival. The next day the shoe-maker, in whose house the family had sojourned for the night, was attacked with cholera. As soon as it became known, the greatest consternation prevailed throughout the village; all work ceased, and people shut up their houses. I hastened to the place, and with considerable difficulty I could procure a bed for myself. The case lasted more than two days; the man appeared to die of the consecutive fever, he was quite alone in the house, and no one, except myself, would approach him: he had no attendance whatever. A grave was dug immediately, I placed the body in the coffin, and drew it to the door, where, for a considerable reward, a man placed it upon a car; it was then brought to the graveyard and buried, about six hours after death. The windows of the house were then burst in, and a quantity of unslacked lime thrown in through these apertures, until the bed, and all the furniture of the room in which the man had lain, were consumed; the door was locked, and no one allowed to enter, I believe, for the next fortnight or three weeks. No other case of cholera occurred in this village or district afterwards."

The above short narrative of the circumstances which accompanied the outbreak of cholera in the village of Kilmaine is very instructive, and the favourable result of the precautions

so promptly taken ought to encourage others to adopt similar measures whenever a case of cholera occurs in any place. It is true that a practitioner who thus acts incurs the fearful responsibility of disobedience to the orders of the powers that be; but if he nevertheless succeeds in preventing the spreading of the disease, the satisfaction he will feel may be considered as a sufficient compensation for any damage his success must necessarily inflict upon the character for infallibility, so modestly assumed by the Boards of Health, both in London and Dublin. It gives me sincere pleasure to find that the enlightened physicians of Belfast have refused the guidance of that official dark lantern,

"Which none see by, but those that bear it"(*a*),

and availed themselves of the means reason and experience point out as most likely to be serviceable at such an emergency. Accordingly, on the arrival of an individual from Glasgow, who had contracted the disease in an infected house in that city, they immediately placed him in a separate ward, with special nurses to attend him, and on his decease they caused all his clothes, and the bed and bedding, to be burned.

Art. II.—*The Results of some Experiments respecting the Presence of Urea in the Liquor Amnii and Fœtal Urine of the Human Subject.* By Alfred H. M'Clintock, M. D., F. R. C. S. I., Ex-Assistant of the Dublin Lying-in Hospital; Lecturer on Obstetric Medicine at the Medical School, Park-street, &c.

In the month of January, 1846, I examined the body of a male child, who had died a few moments after birth at the full term. Externally it presented no unusual appearance, except that the abdomen was rather tumid, and upon cutting into it there

(*a*) Hudibras.

was found in the right side a large cyst containing a transparent straw-coloured fluid. For a moment Dr. Denham (who was assisting at the autopsy) and I were at a loss to imagine what this was, but upon further scrutiny we discovered it to be the *right ureter* most enormously distended and very much convoluted. In some parts its diameter was fully an inch and a half, and no where less than one. The ureter of the opposite side was in the same condition, and the two together, from their great size and tortuosity, occupied a very considerable space in the abdomen. The urinary bladder was perfectly empty, and, as may be anticipated, was very small and contracted. The urethra was pervious throughout its whole course. It is much to be regretted that the renal secretion found in the dilated ureters, and which in quantity could not have been less than a pint and a half, was not submitted to chemical examination; its sensible qualities, however, were these: it was almost devoid of smell, had a slightly saline taste, and, as before remarked, was perfectly translucent and of a pale-yellow colour. It is almost superfluous to mention that the vesical extremity of each ureter was imperforate.

This is by no means a unique case, but its history is recorded as it affords corroborative evidence of the fact that the kidneys commence the office of secretion some time prior to birth; and also because it led to those experiments, the results of which form the substance of this paper. From the careful consideration of a few instances of a similar nature to the above, Dr. R. Lee arrived at the conclusion that the secretion of urine goes on for a considerable period antecedent to parturition at the full term(*a*); which opinion, I need hardly say, is directly at variance with that generally entertained by obstetric writers at the present day. How this notion, that the kidneys were inactive during intra-uterine life, could have orignated, or have

(*a*) Dr. Lee's paper, "on the Functions of the Fœtal Kidney," will be found in the nineteenth volume of the Medico-Chirurgical Transactions.

received currency, I cannot imagine, as every one at all engaged in the practice of midwifery must have over and over again observed infants to void urine, and sometimes very freely, immediately after birth; besides this many of the older physiologists believed that the urine of the fœtus constituted a large proportion, if not the entire, of the liquor amnii. (*Deusing Arantius, Carpensis, Riolan*(*a*)).

There are now several recorded cases of congenital obstruction in some part of the urinary conduits giving rise to an unnatural accumulation of the renal secretion. Thus Dr. Lee, in his paper already alluded to, has brought together seven examples of this malformation. Dr. Fearns has described one that he met with in his practice(*b*); and Dr. Montgomery has given the account of another that came under his own observation(*c*); and it is very possible that a diligent search through the works of authors might discover other instances.

II. With a view to obtaining further information respecting the secretion of urine before birth, Dr. Lee had a specimen of liquor amnii, which he drew off with a canula from a woman six months pregnant, analysed by Drs. Prout and Bostock, but they could not discover any traces of urea, or uric acid, in it, upon which Dr. Lee remarks: "This observation renders it probable that at the sixth month of utero-gestation a very small quantity, if any, urine is formed by the fœtal kidney, and passes into the amnion." Several experimenters have analysed the fluid contained in the sac of the human amnion, but with very opposite results as regards the presence or absence of urea. It may be well, therefore, to take a hasty glance at the observations that have been made towards deciding this point, as their results will be seen to bear a close relation to the further steps of the present inquiry.

The experiments of Fromhertz and Gugert have been fre-

(*a*) Haller, *Elementa Physiologiæ*, tom. viii. lib. xxix.

(*b*) Lancet, May 9. 1835.

(*c*) Cyclop. of Anat. and Physiol. Art. "Fœtus."

quently adduced as proving the existence of urea in the liquor amnii of the human female; but, with Berzelius and Raspail, we think their accuracy on this point may be fairly called in question, as it would appear they were content to infer the nature of the saline product solely from its physical characters. After describing the mode of analysis pursued by them, Berzelius writes: "La decouverte de l'urée dans ce liquide serait incontestablement un fait trés remarkable; mais les auteurs n'ont pas indiqué une seule reaction qui preuve que ces cristaux n'etaient point un depot de nitrate alcaline produit aux depens d'un lactate decomposé, semblable à celui qu'on obtient des liquides de la viande"(*a*). Raspail also, upon the same experiments, thus comments: "Fromhertz et Gugert disent y avoir trouvé une matière salivaire, sans doubte parce qu'ils ont pensé que l'embryon crachait dans la liquide; puis de l'acide benzoique et de l'urée, decouverte qui preuverait que l'embryon humain urine dans la capacité de l'amnios. Mais la premier assertion est une hypothuse et peut être la seconde est une erreur. Il serait possible que l'urée et l'acide benzoique des auteurs ne fussent que des produits illusoires de l'operation"(*b*). We have already seen that Drs. Prout and Bostock failed to detect any urea or uric acid in the specimen of liquor amnii furnished them by Dr. R. Lee; but then it must be remembered that the patient this was taken from was only six months advanced in pregnancy, which circumstance tends to lessen the value of the negative result obtained. Voigt looked for urea in vain; but then it does not seem that he examined the amniotic fluid later than the sixth month of pregnancy(*c*). Mack operated, and apparently with much care, upon three specimens from women at the full time, but could not find it, and suggests that the discrepancies in the results of other chemists may be owing to their having examined the fluid mixed with blood mucus, or urine from

(*a*) *Traité de Chimie*, tom. vii. p. 566.

(*b*) *Nouveau Système de Chimie Organique*. Paris, 1838.

(*c*) Simon's Animal Chemistry, edit. of Sydenham Society, vol. ii. p. 360.

the mother's bladder(*a*). Colberg gives two analyses of human liquor amnii, one contained no urea, and the other only 0·5 in the 1017·7 parts(*b*). In the liquor amnii of a mare, which Voigt examined, he found no urea, nor could Prout or Lessaigne detect its presence in the amniotic fluid of cows(*c*).

So far then the weight of evidence is decidedly *against* the presence of urea in this secretion. On the other hand Dr. Rees has published the results of four analyses of human liquor amnii, in each of which he states that urea was found. "Urea," he adds, "was proved to exist in the liquor amnii by its characteristic crystallization with nitric acid, and the reactions of its nitrate. A further evidence was obtained in the analysis of specimen 3, by the crystallization of chloride of sodium in octohedra, from the alcoholic extract, when allowed to evaporate spontaneously"(*d*). Three of the women from whom his specimens were taken were seven and a half months gone with the child, and the other "appeared to be at about the seventh or eighth month of utero-gestation."

Being desirous to throw, if possible, further light upon this disputed question, I obtained some specimens of pure liquor amnii, from women at the full term, and submitted them for analysis to Dr. William Moore, of this city, whose zeal and success in the cultivation of this department of chemistry pointed him out as a most fit person to conduct these examinations. It may be well to premise that in every instance the strictest precautions were employed to procure the amniotic liquor as free from any extraneous matter as possible, and in no case was there a specimen used of whose perfect purity any doubt could be entertained. I shall now give the detailed account of the experiments, in Dr. Moore's own words:

(*a*) Simon's Animal Chemistry, vol. ii. p. 361.

(*b*) Ibid. p. 451.

(*c*) Perhaps the existence of the allantois may account for the absence of urea in the liquor amnii of these animals.

(*d*) Guy's Hospital Reports, vol. iii. p. 396.

"Specimen 1.—About three ounces, furnished to me August 18, 1848. It was found to be faintly alkaline; specific gravity 1·006. Under the microscope it presented numerous oil globules and much epithelium. Boiling separated a large quantity of albumen; the filtered liquor was slowly evaporated on a sand-bath, some dilute nitric acid having been previously added; on cooling, crystals were obtained, which to a practised eye, however, bore no resemblance to nitrate of urea; heated in the flame of a spirit lamp, they frothed up and became brownish, they then fused without emitting any animal odour, and, when they had cooled, were perfectly white. They were evidently fixed inorganic salts.

"Specimen 2 was treated according to the plan recommended by Fromhertz and Gugert, viz., it was evaporated to the consistence of a syrup, treated with hydrochloric acid, and filtered. Nitric acid was then added, and the whole exposed to the action of a freezing mixture, but no crystals of nitrate of urea were obtained.

"Specimen 3, in quantity about two and a-half ounces, was faintly alkaline; specific gravity, 1·007. It exhibited under the microscope oil globules, blood, and a very large quantity of epithelium. Evaporated slowly on a sand-bath, a membranous pellicle constantly formed, which was from time to time removed. When concentrated it was treated with nitric acid and exposed to cold, but afforded no crystals of nitrate of urea.

"Specimen 4 was examined November 22, 1848. It was highly albuminous, had an alkaline reaction, and its specific gravity was 1·006. Viewed through the microscope it presented a number of fat globules and a large quantity of epithelium. A portion of it, nearly equal to half a pint, was evaporated in a water-bath to the consistence of a syrup; alcohol was then added and the alcoholic solution was filtered and evaporated in the water-bath nearly to dryness; the addition of a few drops of water, and subsequently of a few drops of nitric acid, was now made. The mixture was allowed to

stand all night in a laboratory, the temperature of which at eight o'clock next morning was 45°. No crystallization took place, nor could any be discovered, even with the aid of the microscope.

" Specimen 5. December 1, 1848. Eight ounces of liquor amnii were evaporated in a water bath, to the consistence of a syrup, alcohol was then added, and the alcoholic solution filtered and evaporated in the water bath nearly to dryness; a few drops of water, and afterwards of strong nitric acid, were added; the mixture was exposed in a watch-glass, during the night, to a low temperature: at the end of fourteen hours no trace of nitrate of urea could be discovered with the microscope."

The negative result of these analyses, and of two or three others, the particulars of which we have not thought it necessary to record, surprised us not a little, as, when commencing the investigation, we expected to have been able to discover traces of urea in the amniotic fluid.

III. Here then, it will be perceived, were two classes of facts seemingly in opposition to one another, viz.: first, the undoubted proofs of urine being secreted by the fœtus in considerable quantity before birth; and secondly, the complete absence of urea in the majority of instances where the liquor amnii was subjected to accurate chemical analysis. In order to reconcile these discordant conclusions, it became an object of the utmost importance to ascertain the composition of the fœtal urine, as hitherto observers appeared to have taken it for granted that the renal secretion possessed the same chemical characters during intra-uterine life, as it does after birth. Accordingly this was the next point to which our attention was directed.

Dr. Prout, at the request of Dr. R. Lee, analyzed some of the renal secretion found in Mr. Hay's case. After the separation of the albumen contained in it, he treated it with alcohol: " That menstruum was found to take up a principle strongly acid, and which readily assumed an imperfect crystallized form. I cannot venture" (he says) " to give this principle a

name; it somewhat resembled the acid called amniotic, or rather allantoid, in some of its properties, but differed from it in others. The alcoholic solution gave at first faint and somewhat doubtful traces of urea; on standing several days these became very distinct. After the albuminous matter had separated, ammonia produced a deposition of triple phosphate"(*a*). In a case of nearly the same nature communicated to Mr. Howship, by Sir B. Brodie, the urine was examined by Mr. Brande, "who found it to have the other properties of urine, but to have no uric acid in its composition"(*b*).

With the exception of these two, I have not been able to find that any analysis of this fluid has been made. I again had recourse, therefore, to the services of my friend, Dr. Moore, who, with his usual kindness and promptitude, offered to undertake a series of experiments upon the fœtal urine. To procure specimens of this, however, was the great difficulty, as infants at birth will not void urine at our bidding, and it did not seem altogether justifiable to have recourse to the catheter in order to obtain it. The only way, therefore, in which I could accomplish my object was, to remove the contents, if any, of the urinary bladder from still-born children, whenever opportunity afforded for so doing. Each of the children from whom the specimens were taken, had attained the ninth month of utero-gestation. I now subjoin the details of Dr. Moore's experiments.

"The first specimen of fœtal urine which I examined was mixed with blood, and was albuminous. Concentrated and treated with nitric acid, it afforded no urea. The quantity operated on was only three drachms; not sufficient to take the specific gravity with Newman's bottle.

"No. 2, September 8, 1848.—Was free from blood, but highly albuminous, and exhibited under the microscope much

(*a*) Dr. Lee's paper before quoted.

(*b*) A Practical Treatise on the most important Complaints that affect the Secretion and Excretion of the Urine, p. 376. London, 1823.

epithelium and numerous mucous granules. Concentrated, filtered, treated with a small quantity of strong nitric acid, and exposed to the action of a freezing mixture, no crystallization took place. The quantity operated on was about half an ounce.

"No. 3, September 28, 1848. Neutral and albuminous. Examined with the microscope, mucous granules with much epithelium and some amorphous matter were observed. During evaporation a membraneous pellicle formed, and afterwards a precipitate was produced by the addition of nitric acid. Having been concentrated, a few drops of strong nitric acid were added; it was then filtered and further evaporated, and exposed to the action of a freezing mixture, but no crystallization took place.

"No 4, October 31, 1848. Urine drawn off with a catheter from the bladder of a still-born child born in a state of incipient putridity. It was highly albuminous, was free from blood, and deposited epithelium and amorphous matters.

"No. 5, October 31, 1848. Urine drawn off this day from a still-born fœtus, which had been very recently alive. The quantity was so small (about a fluid drachm) that I was only able to ascertain that it was highly albuminous.

"Nos. 4 and 5, it will be seen, were drawn off with a catheter, in order to prevent the possible admixture of blood or serum. The former specimens were obtained by exposing the bladder and carefully removing its contents through an incision, but the quantity of albumen present in the specimen which contained blood, was far too great to have been derived from this source.

"No. 6. A very small quantity of urine passed by a male child immediately after birth was found to be highly albuminous.

"November 7, 1848. The urine of a fœtus which perished during labour, owing to prolapse of the umbilical cord, and which had been alive three hours before, was found to be albuminous and to have an acid reaction. A portion measuring

half an ounce was evaporated, and when highly concentrated was treated with nitric acid. No crystallization ensued.

"Having thus unexpectedly failed in discovering the presence of urea, by acting with nitric acid upon the urine when highly concentrated, I determined to try the method of detecting it by means of alcohol, in order to effect a more complete separation of the albumen before adding the nitric acid, and thereby to render the detection of urea, if present in very minute proportion, more certain. The following specimen having been obtained in much larger quantity than any of the preceding, afforded a good opportunity for carrying out this plan, and of testing the accuracy of the previous results.

"No. 8, November 27, 1848. An ounce and a half of urine taken this day from the bladder of a fœtus at the full time, which had perished during birth, the face having presented, was found to be acid, and its specific gravity 1·0085. It was evaporated in a water-bath and during this process a quantity of albumen was coagulated. When at a high degree of concentration a small portion was removed with a pipette and placed in a watch-glass; to this a few drops of strong nitric acid were added, which produced a fresh coagulation. At the end of twelve hours no crystallization was visible, nor could any be observed with the aid of the microscope.

"The evaporation of the remaining portion was continued nearly to dryness, when a quantity of strong alcohol was added. To separate some more coagulated albumen, the alcoholic solution was filtered, and again evaporated nearly to dryness, whereupon a few drops of water, and subsequently a few drops of strong nitric acid, were added. The mixture was allowed to stand all night in a laboratory without a fire, and at the end of twelve hours no crystals could be discovered, either with the naked eye, or with the assistance of a powerful microscope.(*a*)

(*a*) Whilst these sheets were going through the press, Dr. Johnson handed me the copy I had sent him from my note-book of an examination made in the year 1838, which I had entirely forgotten till thus reminded of it. As it di-

"It is stated by very high authority that 'infants secrete scarcely a trace of urea'(*a*), and if this observation refers to the first few weeks of existence, it tends to corroborate and strengthen the results of the above experiments, at least in so far as regards the absence of urea. At a somewhat more advanced period of life the urine does not present any very remarkable difference from that secreted by the adult, as may be seen from the following experiments, which will be found interesting in connexion with this subject.

"No. 1. The urine of a healthy female infant, aged six months, and nourished entirely at the breast, was acid and free from albumen. Specific gravity, 1·007. When concentrated and treated with nitric acid, it formed numerous distinct crystals of nitrate of urea.

"No. 2, October 7, 1848. Urine passed yesterday by a child aged ten months, labouring under dysentery. The food previously to yesterday, consisted of biscuit powder, isinglass, and arrow root. It had got two doses of a mixture of blue pill, mucilage, syrup of orange-peel, and tincture of opium. This urine was found to be acid; specific gravity 1·012; deposited a large quantity of urate of ammonia; contained no albumen; urea was present in small proportion. N. B.—This urine had been passed twenty-four hours before it was examined, the weather being at the time warm. On the 7th October the ther-

rectly bears on the subject before us, I transcribe the entry as it stands in my note-book: "Examination of fluid found in large quantity (about six ounces) distending the bladder, ureters, and kidneys of a fœtus which died immediately after birth, and in which the urethra was imperforate. I do not find any analogy between it and urine, except that it contains some of the salts found in urine. It appears to be a serous fluid, tinged with the colouring matter of the blood; specific gravity 1·003; contains a considerable quantity of albumen; and has a slight alkaline reaction when concentrated, but not otherwise; boiled and filtered it affords precipitates with muriate of barytes and nitrate of silver; it is very slightly clouded on the addition of ammonia; I cannot discover a trace of urea."

(*a*) Sir Robert Kane's Elements of Chemistry, p. 1165. Dublin, 1841.

mometer stood at 63° in a room where there was no fire, and to which the sun had not access.

"No. 3, October 9. Urine of a healthy female infant, aged six months; spoon-fed, diet consisting of arrow-root, barley water, milk, &c. It contained no albumen, was acid, and the specific gravity was 1·007. When concentrated it afforded, on the addition of nitric acid, an abundant crystallization of nitrate of urea. Here then we have three specimens of urine from children under a year old, one of whom was nourished exclusively from the breast, another was fed artificially, and the third was labouring under disease; and in each instance the quantity of urea present was pretty considerable, fully proportionate to the specific gravity of the fluid operated upon."

The above experiments are certainly most interesting, but in point of importance and novelty cannot compare with those on the fœtal urine, already detailed. The results of these latter were indeed most unlooked for, and, as far as they go, amply justify the deduction, that the urine of the child before birth differs in its chemical constitution from that secreted subsequently, in two very remarkable particulars, viz.: first, in containing a large quantity of albumen; and secondly, in being almost, or altogether, deficient in urea.

The liquor allantoidis of some of the lower animals, the cow for example, is considered to be the urine of the fœtus; but although it has several times been the subject of chemical investigation, urea has not as yet been detected in it, and albumen has always been found present. This would seem to give additional confirmation to the results we have obtained in our examinations of the fœtal urine of the human subject. According to the views of Liebig, the peculiar substance called *allantoin* appears to replace both urea and uric acid in the urine of the fœtus of brutes(*a*); it would, therefore, be an interesting subject for further inquiry to ascertain whether this

(*a*) Simon's Animal Chemistry, vol. i. p. 57.

or some analogous product be present in the urine of the human fœtus.

That there should be this striking dissimilarity in the composition of the urine secreted before and after birth, cannot so much surprise us when we reflect upon the totally altered position, so to speak, in which the living fœtus is placed by the act of parturition. Instead of receiving blood from the mother's system fully elaborated and prepared for the purposes of nutrition, this fluid must henceforward be eliminated within its own body; and accordingly with independent life commence all those vital processes which have for their object the elaboration of the blood, and foremost amongst which must rank digestion and respiration. Now we know that the former of these exercises a marked influence over the secretion of the kidneys; and it is still a question whether nitrogen (a principal constituent of urea) be absorbed by the lungs during respiration. Again, we may safely assume, that nature, who does nothing imperfectly, transmits the blood to the fœtus in a state of the highest purity, and containing no useless compounds or superfluous elements. All these things considered, then, we cannot wonder that the secretion of the kidneys should be found to present such very different characters during *dependent* life, from what it does during *independent* life; but why this difference should consist chiefly in the two points already stated, is a question upon which we do not feel competent to pronounce, but leave it to those whose particular studies and pursuits render them more intimately acquainted with this department of organic chemistry. We feel assured, however, that the results of the foregoing experiments are not mere barren facts, devoid of application or utility, but on the contrary, that they are calculated to throw considerable light upon the source of urea, and also upon the relations between this substance and albumen in the animal economy.

It is most curious and interesting to observe, that there is one morbid condition of the kidneys in the adult,—albuminu-

ria,—in which the urine re-assumes the characters belonging to it during intra-uterine life, in becoming albuminous, of low density, and deficient in urea. In this disease M. Solon has conceived "that the albumen might be formed by a sort of conversion, at the expense of the urea; since these substances, by a slight alteration in the ratio of their elements, pass respectively each into the other"(*a*). A similar mode of reasoning might, perhaps, apply in the case of the urine of the fœtus, whose system contains little nitrogen, and where consequently it would appear more natural for albumen to replace urea as an excretion. The following analyses, taken from Turner's Chemistry, will show the chemical composition of these two substances.

UREA.	ALBUMEN.	
	Gay Lussac and Thenard.	Prout.
Carbon, 2 equivalents.	17 equivalents.	15 equivalents.
Nitrogen, 2 ,,	2 ,,	2 ,,
Hydrogen, 4 ,,	13 ,,	14 ,,
Oxygen, 2 ,,	6 ,,	6 ,,

To close this communication without expressing my sincere thanks to Dr. Moore, and to Dr. Sibthorpe, Assistant at the Lying-in Hospital, would be an unpardonable omission. The truly valuable assistance lent me by the former gentleman, has already been seen(*b*); and to the latter I am indebted for most of the specimens of liquor amnii and fœtal urine, without which our researches would unavoidably have required a much greater length of time for their completion.

(*a*) Watson's Lectures on the Practice of Physic, first edition, vol. ii. p. 577.

(*b*) I am happy to be able to state that Dr. Moore is at present engaged in a series of experiments upon human milk, the results of which, when fully completed, will be laid before the Profession.

Art. III.—*Observations on some practical Questions in Surgery: 1. On the Operation for tying the Subclavian Artery, internal to the Scaleni Muscles; 2. Luxation of the great Toe upon the Dorsum of its Metatarsal Bone, which presented unexpected Difficulties in the Attempt at Reduction.* By William Hargrave, M. B., Professor of Surgery to the Royal College of Surgeons in Ireland, Surgeon to the City of Dublin Hospital.

I. ON THE OPERATION FOR TYING THE SUBCLAVIAN ARTERY INTERNAL TO THE SCALENI MUSCLES.

The statistics for securing the right subclavian artery in the first part of its course, or internal to the scaleni muscles, afford a melancholy, and almost desponding history of this operation, as to its ever being successful. Every case on record (ten) of securing this vessel, has been a failure, whether performed in Great Britain, in Ireland, in the colonies, or elsewhere ; to all the same fatal termination—death, and all by the same fatality —hemorrhage. Are surgeons to abandon this operation in future (Liston says that it should never be again performed(*a*))? or are they, in the spirit which should always actuate those engaged in our profession, to seek for the causes of failure? If these can be discovered, a way will then be opened which will lead with greater certainty to success.

The causes of the failure in this operation appear to me to be owing to the four following sources: 1st. To the anatomical position and relations of the vessel. 2nd. To the great disturbance, perhaps destruction, of the nutrition of the artery, caused by the difficulty of the operation. 3rd. To the tension of the artery after the ligature is tied. 4th. To the pathological conditions of the artery itself.

1st. As to the anatomy of the vessel and its relations, in-

(*a*) Liston's Surg., fourth ed., p. 200.

dependent of the intricacy of the latter, and the consequent difficulty in laying it bare, it differs in one respect from every other artery in the body which is required to be secured in its continuity, namely, after the artery is tied, it can be relaxed by position, which allows of great repose and a certain amount of quiescence, and so removes any strain caused by the ligature on it; this desideratum cannot be obtained for the subclavian, in consequence of the presence of the clavicle, which offers an insurmountable resistance to relaxing the artery after being secured. Place the extremity in any position, still no relaxation will follow, the consequence of which is that the vessel is kept in a state of tension, in all probability increased by the ligature.

2nd. The disturbance of the nutrition of the artery, perhaps its destruction, by which I indicate the injury inflicted upon its vasa vasorum, and the small quantity of cellular membrane in which it is imbedded; the often tedious proceeding—in many cases inevitable, to expose the artery with security to the important parts in the vicinity of which it is situated, destroying too many of the nutrient vessels, and thus depriving the artery of the sources from which the processes necessary for the effusion of plastic lymph and of adhesion are obtained. It is this injury inflicted upon the nutritious arteries of the vessel, which favours the occurrence of hemorrhage, and prevents the effusion of lymph where the ligature has been applied, which is corroborated by the condition of the vessel after death.

3rd. The increased tension of the artery should be always kept in mind. The effects of this cause are well established in Dr. Hayden's case, "The artery at the site of the ligature was gaping irregularly for three-fourths of its caliber, one-fourth sound and retaining the ligature;" and in Mr. O'Reilly's case "the divided extremities of the artery were patulous, and separated nearly two inches by coagula; their edges were jagged and irregular, and there seemed to have been not the slightest attempt at the reparative process;" the arteria innominata was

sound, and the heart natural. What reparation could have filled an arterial space of two inches?

4th. If the artery is diseased at the place where it is tied, failure is inevitable.

Such seem to me to be the causes which mainly act in rendering the operation, as hitherto performed, unsuccessful; can any of them be obviated? I incline to the opinion that the chief one can, namely, the impediment to a quiescent, if not an absolute state of repose for the artery, the want of which has been already pointed out.

It has been proposed, to facilitate the operation for securing the vessel in question, "to saw the clavicle"(*a*), which would certainly afford more space for the subsequent dissections; this suggestion is borne out by performing the operation on the dead body: Cruveilhier has also advocated such a practice.

It is this step in the operation which I would again propose, not so much for the facile exposing of the artery, but to allow it to be gently relaxed, after having been secured, the section of the clavicle would allow this to be done by permitting the approximation of the shoulder to the trunk, and so remove any strain or tension that the ligature might cause on the vessel; it would also remain more imbedded in the surrounding cellular membrane, and receive its supply of blood more freely to assist in the sanatory processes consequent on the operation; while the movements of the upper extremity would produce but little, if any, disturbing effects upon the artery.

The mode of conducting the operation I propose would be, after the vessel was exposed and encircled in the ligature, carefully to saw through the clavicle about its middle, having previously guarded the subjacent parts with a spatula. If any alterations followed this step in the relations of the artery they would be of little consequence, it being noosed prior to the section of the bone; no delay or hinderance would then prevent

(*a*) Vide my Operative Surgery, p. 44.

the tying of the artery. The action of the muscles which draw the shoulder to the trunk, as the subclavius and pectoralis minor should then be aided by position, and the arm retained *in situ* by a bandage.

I have attentively sought for the published opinions on this proposal for sawing the clavicle, and can find but the following: in Velpeau's Operative Surgery he states, "that he can scarcely comprehend the reasons which M. Cruveilhier assigns as being useful to saw this bone, to tie the subclavian artery with more success"(*a*). The work referred to by Velpeau I have never seen, nor can I find it in the library of the Royal College of Surgeons, or in Trinity College.

This proceeding is spoken of in the following words by Dr Norris, of the Pennsylvanian Hospital, U. S., in a very valuable memoir on the mortality following the tying of the subclavian artery: "In cases of great difficulty of passing the ligature around the artery, it has been proposed by Mr. Hargrave and M. Cruveilhier to saw through or excise a portion of the clavicle,—a procedure, we should suppose, which would greatly tend to increase the difficulty of the operation"(*b*). It is evident from this quotation he refers to the operation for tying the artery in any part of its course. I confess that I am sceptical as to its adding to the "danger of the operation" in any part of its course, while I contend for the practice, especially as to the artery in the first part or its internal third.

The next authority which I can find is that of Dr. Flood, in his truly practical work on the arteries(*c*), who says, in referring to this proposal, after quoting the passage in all its details from my work, "they are entitled to the most serious consideration." Neither Velpeau nor Dr. Norris states explicitly their objections to this proposition.

Finally, in reference to the propriety of appealing to ope-

(*a*) *Etud. Anatom.* tom. ii. p. 609.

(*b*) American Jour. of Med. Sci., No. xix., New Series.

(*c*) Flood on the Arteries, p. 88.

rative surgery for the relief of aneurism of the subclavian artery, owing to the fatality of securing the vessel in the first third of its course, it has been suggested to "amputate at the shoulder-joint, treating the stump in the ordinary manner, and keeping up steady and properly regulated pressure on the disease"(*a*).

In the two unsuccessful operations on this vessel, I quote these cases from Dr. Norris's paper, and consider that they were performed external to the scaleni muscles,—one by Sir A. Cooper, the other by Dupuytren. The former abandoned the operation, as he found it impossible to pass the ligature without including some of the brachial plexus; the latter, after an operation of one hour and forty-eight minutes' duration, succeeded, as he thought, in passing the ligature around the subclavian artery from above the clavicle. Pulsation continued in the tumour after the operation, and death took place on the ninth day, when the ligature was found loosely knotted on that portion of the fourth cervical which afterwards becomes the external cutaneous or musculo-spiral nerve. Dupuytren states that it was the most tedious, difficult, and painful operation that he had ever attempted. In these cases, had the clavicle been divided, the one would have been enabled to secure the vessel, the other also to effect this object, and not to mistake and tie a nerve in place of it.

In a medico-ethical consideration, the failure of operations in such hands as I have just quoted may be productive of unpleasant results; by which I intend to convey that failure by such practitioners might induce some surgeons rashly to undertake hazardous operations, when, if unsuccessful, they might console themselves and reconcile their want of skill by a reference to similar errors in the authorities now mentioned.

The object which should always be kept in view, and one of the fundamental principles of surgery, is to arrange its ope-

(*a*) Fergusson's Surgery, second edition, p. 441.

rations for their successful performance, not so much for the naturally skilful, but more for those who are not so gifted, who, by pursuing correct indications laid down for them, will be able, with only moderate dexterity, to perform their operations with present safety, and ultimate success to the patient.

II. LUXATION OF THE GREAT TOE OF THE LEFT FOOT ON THE DORSUM OF ITS METATARSAL BONE, WHICH PRESENTED UNEXPECTED DIFFICULTIES IN THE ATTEMPT AT REDUCTION.

Much as has been accomplished of late years by surgeons, both British and foreign, to render perfect our knowledge of the very important and frequent class of accidents termed dislocations, for which correct and accurate information is of such importance to the practitioner, that whatever can elucidate or afford more precise views of their nature and the impediments to their reduction, should induce every one to add his contribution to the profession for that object. Influenced by these feelings, I am desirous to record the following case, as presenting some peculiarities in the dislocation of the great toe on the dorsum of its metatarsal bone heretofore unnoticed, as far as my inquiries enable me to ascertain.

Charles Pike, a farm labourer, aged 23, was admitted into the City of Dublin Hospital November 27, 1846(*a*), suffering from luxation of the great toe of the left foot upon the dorsal aspect of the corresponding metatarsal bone, and with fracture of the fibula of the same side, a little above the malleolus. The accident was caused by direct violence, he having been thrown from a horse on the kerb-stone of a footway, the horse falling upon him.

Symptoms of the luxation were so evident as to be almost immediately detected; the riding of the great toe upon the

(*a*) This patient was admitted under Mr. Orr, and transferred to Dr. Williams; with their consent I publish it, the details of the case having been noted by Mr. T. D. Hargrave, L. R. C. S.

metatarsal bone; the prominence caused by it; the shortening of that section of the foot; not alone apparent, but rendered more so by comparison with the uninjured one; the motions most imperfect, indeed of no account: the plantar aspect of the ball of the toe was fuller than natural.

Reduction of this luxation was attempted by a well-applied lac on the toe, a good purchase being made on it, the foot having been previously well and firmly secured. The extending power was first made in the axis of the luxated bone, pressure, at the same time, being made on the phalangar end of the toe which rested upon its metatarsal bone, and was so prominent as to afford a good fulcrum to act on. After persevering in the attempt for a reasonable time, and no success attending on it, the toe, still extended, was drawn to nearly a right angle with the foot, and pressure applied to its base, the two being then gradually brought to the horizontal position. This attempt also failed; further endeavours at reduction were then abandoned.

After the lapse of a few days reduction was again attempted, this time being aided by tenotomy of the extensor proprius policis pedis and internal tendon of the extensor digitorum brevis, also by the free division of the fibrous tissues on the internal surface of the joint, but no impression whatever was made in effecting it: the attempt was then finally abandoned.

In the interval which elapsed since the last attempt at reduction in December, an abscess formed in the ball of the toe, which burst, leaving a fistulous opening, and a fistula also formed at a corresponding point of the dorsum of the toe, through which latterly a probe could be passed so as to reach the bones, which felt denuded; likewise, when the toe was moved on the metatarsal bone, a grating sensation was felt: *the toe was in the flexed position.*

This patient now came under the care of Dr. Williams, and on February 24 he performed the following operation to remove the articular surfaces of the phalanx and metatarsal

bones: prior to it the patient was submitted to the influence of ether, and continued under its agency till it was completed. An incision was then made parallel to the inner edge of the foot, met by another at right angles, carried transversely outward over the line of the articulation. On dissecting away the integuments, the phalanx was found resting upon the superior and external part of the metatarsal bone, being firmly lodged in the interval between the first and second metatarsals. The capsular ligament did not seem opened on its anterior internal aspect, but a probe, passed into the superior fistulous opening, got into the joint, which was next opened from above.

Condition of the ends of the bones:—a small portion of the cartilage of the head of the metatarsal bone, on its superior aspect, was absorbed, equal to a split pea, without the presence of pus, while that portion of the bone on which the phalanx rested was soft and carious. The articular surface of the phalanx was covered with its cartilage, and appeared healthy; the sesamoid bones were also healthy, and fixed in their natural position. The articulating surface of the phalanx was then removed with a strong-bladed scissors, next the head of the metatarsal bone, including the carious portion already noticed, which, owing to its softness, was easily excised with the same instrument. The parts were then adjusted, the wound cleansed, dressed simply, and the patient carried to his bed.

Nothing worthy of note occurred in the healing of the wound, and, on the 10th of April he was permitted to leave his bed and walk about the ward, supported by crutches, but not to bear upon the toe. On the 7th of May succeeding he was discharged from the hospital in the following condition: he possessed the power of *extension* and of flexion of the toe; he could bear his weight firmly on the posterior part of the foot, but did not venture on the ball of the toe.

I have had many opportunities of hearing of the progress of this very valuable case since he left the hospital, and am in-

formed that he has completely regained the use of his foot and toe, and is fully able to perform all his duties as an active farm servant.

This luxation presents many points of analogy with that of the first phalanx of the thumb on the dorsum of its metacarpal bone. First, as to the anatomical relations, the articulating heads of the metacarpal and tarsal bones are both very large, each possessing a more or less well-formed neck ; each is furnished with two well-marked and fully developed sesamoid bones, into which a number of powerful muscles are inserted. Into the sesamoids of the metatarsal bone, the internal one has attached to it the abductor pollicis pedis and the inner tendon of the flexor brevis pollicis pedis ; the external one has, inserted into it, the external tendon of the flexor brevis, the adductor pollicis pedis, and a portion of the transversus pedis: the lateral ligaments of the articulation are strong and firm, while the capsular one is particularly so on its plantar aspect. The analogy between these two articulations is still carried out in reference to the articulating surface of the first phalanx of the toe, which presents a very superficial cavity, having but a small proportion to that of the corresponding head upon which it moves; and very strong muscles inserted into it, viz., the internal tendon of the extensor pollicis brevis, and the tendon of the extensor proprius pollicis pedis. On the inferior surface of the last phalanx the tendon of that very powerful muscle, the flexor longus, strengthened by its connexion with the flexor digitorum longus communis is inserted into it, while to the internal surface of the first phalanx the abductor pollicis pedis is attached. Taking the two bones constituting the toe, we have four muscles attached to them, three of which are very powerful,—the proper extensor of the great toe, the long flexor of the great toe, and the abductor.

What was it that opposed the reduction in this case? Nothing but muscular agency, as already indicated. A sufficient purchase was had by the lac, so carefully applied as would have

enabled the phalanx to be torn from the foot, if required. By tenotomy the extensor tendons were divided, and, I would also say, the abductor pollicis pedis; still reduction could not be accomplished. As far as muscular opposition was further concerned in this luxation, we must look principally, if not solely, to the long flexor, and its connexion with the flexor longus digitorum pedis, which prevented the removal of the phalanx from the abnormal position into which it was forced by the accident. It may be asked, what would the section of the first-named tendon have effected?

One pathological fact, of great value, has been derived from this case, namely, the perfect restoration of the extensor muscles to their proper functions over the phalanges of the toes, after what may be considered a double operation,—the first being that of tenotomy,—the second, the excision of a portion of the phalanx of the toe with the head of the metatarsal bone. This reparation took place in a comparatively short period,—three months; also a healthy false articulation was formed, compensating for the natural one.

If this luxation had been left to the sole efforts of the system, to take its own course, would the results have been as favourable, and so quickly accomplished? I think not; as, from the position of the phalanx, lodged upon and between the two internal metatarsal bones, the same kind of deformity would have permanently followed, which is represented as affecting the metacarpo-phalangar articulation of the thumb in a paper by Mr. Adams in the Cyclopædia of Anatomy, obtained from me. What should be the line of practice in such a luxation? is a question more easily asked than satisfactorily answered. If tenotomy is resorted to, it should include the section of the long flexor of the great toe, and then to attempt the reduction.

I have been able to obtain scarcely any information on this particular accident from any published works: no example in Cooper on Dislocations and Fractures, who, in speaking of dislocation of the toes from the metatarsal bones, says it is a

very uncommon accident. Dupuytren(*a*) is altogether silent on it. Delpech describes this luxation in the following terms, which I transcribe from his work: he says that "the great toe has been luxated on the first metatarsal bone and carried to its external side, while the soft parts on the internal side of the articulation have been lacerated, from which results a considerable projection of the metatarsal bone and the impossibility to reduce the luxation; the denuded bone was disarticulated and removed in totality; the contraction of the muscles drew back the first toe, but not sufficiently far to bring it into contact with the first cuneiform, so much so that this member was useless and without motion, for want of a '*point d'appui*,' and the too great laxity of the muscles. We think that in a similar case it would be better to make the resection of the denuded metatarsal, and to procure anchylosis with the great toe"(*b*). This is evidently a compound luxation; the practice recommended is good, but would fail in effecting anchylosis.

Boyer says the possibility of luxation of the first phalanx of the great toe from the first bone of the metatarsus may be easily conceived. It is not necessary to give here the rules to be followed in such a case; they consist in reducing the luxation and amputating the great toe, when the state of the soft parts renders it impossible to preserve it(*c*). Another example of a compound luxation.—Liston mentions a case of a compound luxation of the distal extremity of the metatarsal bone of the great toe, which was broken; "the broken head of the bone was removed," the patient recovered with the perfect use of his foot(*d*).

In cases of failure of the reduction of this luxation, "the

(*a*) Dis. and Surg. of Bones, published by the Sydenham Society.

(*b*) Delpech, *Precis Elementaire des Maladies réputées Chirurg.*, tom. ii. p. 141.

(*c*) Boyer on the Bones, translated by Mr. Farrell, vol. ii. p 205.

(*d*) Liston's Surgery, p. 137.

projecting end is recommended to be sawn off, and the bone kept in its proper place by a suitable apparatus"(*a*).

From such researches it is evident that the rarity of this accident must be very great, and fully justifies it being brought before the profession. In cases of difficulty and failure in the reduction, the surgeon will either have to perform tenotomy of all the tendons of the toe, which is a simple and comparatively painless operation, even without an anæsthetic agent: or to perform a more compound one, the section of the projecting end of the bone, and then reduce it, and maintain the parts *in situ* by proper bandaging and apparatus. From the success attendant upon Liston's case, also from the one now submitted to the profession, perhaps it would be the most satisfactory practice to have immediate recourse to resection of the bone, after the attempts at reduction had failed, and follow up the treatment so as to obtain a false but useful articulation.

Art. IV.—*Report upon the recent Epidemic Fever in Ireland.*

In the hope of preserving some record of the late disastrous epidemic which devastated this country during the years 1847 and 1848 in particular, the Editor of this Journal addressed a circular to those medical practitioners throughout the kingdom from whom he thought it likely he should obtain the desired information. The list of questions contained in the circular, together with the accompanying letter, which will be found in the note on the opposite page, will best explain the nature and amount of the information sought for.

To this circular more than seventy answers have been received, being in some instances extensive reports, and in others satisfactory replies of shorter extent.

(*a*) Chelius, translated by South, vol. i. p. 814.

The queries issued may be classed under the eight following heads(*a*):—1st, those relating to the history and antecedents of the outbreak of the epidemic, including numbers 1 to 12;—2nd, those describing the character and symptoms of the disease, extending from 13 to 20, both inclusive;—3rd, the complications, including numbers 21 to 29;—4th, the mortality and morbid appearances, including 30 to 33;—5th, the duration of the disease, including 34 and 35;—6th, the modifications, extending from 35 to 38 inclusive;—7th, the sequelæ, comprehended in query 39;—and 8th, the treatment, including the numbers from 40 to 44.

(*a*) The Editor of the Dublin Quarterly Journal of Medical Science presents his compliments to Dr. ———, and begs to solicit his co-operation in bringing out a collective Report on the recent Epidemic Fever of Ireland. With this view he takes the liberty of submitting to him the accompanying list of queries, to each of which, he need hardly observe, separate replies are not expected. They have been merely drawn up as indicating some of the subjects on which information is desired by the profession here, and on the Continent, &c., &c.

1. Has the late epidemic of fever been prevalent in your neighbourhood?

2. What was the state of health of the district previous to the breaking out of the epidemic?

3. What was the date of the commencement of the epidemic?

4. Were those attacked previously in good health or the reverse?

5. What classes in society were chiefly attacked?

6. Did the disease often set in after recovery from the effects of starvation?

7. Did change of diet among the people appear to influence disease?

8. Was the fever frequently preceded by scurvy or purpura?

9. Was there any disease of cattle prevalent at the same time as, or previous to, the epidemic of fever?

10. At what ages were the greatest number attacked?

11. Were males more liable to the disease than females?

12. What was the proportion of cases to the population of the district?

13. Have you evidence as to the disease being contagious?

14. Was there any difference in the symptoms of cases occurring in different classes of society, and, if so, what were these differences?

15. What difference in character was there between cases following starvation, and others not so circumstanced?

To the circulars containing these queries about eighty replies have been already received, including several most valuable and laboured reports ; and from the information thus obtained, the following historical outline of this epidemic fever has been drawn up. In it we propose to give an account of the disease as it affected each province, and also each individual county. With a single exception it has been uniformly mentioned in the replies received that the epidemic prevailed generally throughout the various districts. The solitary exception has been the district of Warrenpoint and Rostrevor, in

16. At what period did well-marked typhoid symptoms follow the starvation state?

17. In what particulars, if any, did this fever differ from former epidemics?

18. Were purpuric spots common?

19. Was crisis common?

20. What were the most usual critical phenomena?

21. Did dysentery frequently precede, accompany, or follow fever?

22. What local complications were most frequent, and mention their comparative frequency?

23. Did relapses occur frequently?

24. What difference was there between the primary and relapsed cases?

25. Did petechiæ ever appear in relapse, when they did not occur in the primary fever?

26. Please to state any morbid phenomena of the heart which were present?

27. Was epistaxis frequent, and at what stage did it generally occur?

28. Was enlargement of the spleen observed in any of the cases, either during or subsequent to fever?

29. Were bed-sores frequent, and what was their nature?

30. What was the proportionate mortality of the various classes attacked?

31. Was there a greater per centage of mortality among those attended at their own houses, or in hospitals?

32. Did the towns and densely populated districts in your neighbourhood exhibit a greater amount of mortality than the thinly populated rural districts?

33. Was there anything remarkable in the *post mortem* appearances?

34. What was the usual duration of the disease?

35. What was the usual length of convalescence?

the County Down, from whence Dr. Ross has forwarded to us the following statement:

"In reply to your favour relative to the late epidemic of fever, I am happy to say that it was *not* prevalent in this neighbourhood, so I cannot give you any information on the nature of it, &c. We have very little fever here at any time, and certainly during the last two years not more than usual.

"As a reason for this I may state, first, that here starvation (or indeed, I may say, want) is seldom or never known; our gentry are numerous, rich, and charitable, and give a great deal of employment to the poor. Secondly, our soil is porous and sandy, so that our roads become perfectly dry one hour after the heaviest rains; and although our air is frequently charged with pure damp from the clouds, we never have that unhealthy and impure vapour which evaporates from the surface of a retentive soil."

It will be perceived from a perusal of the following pages that from several districts of Ireland where the late epidemic committed fearful ravages, no reports have been received. In many cases we regret to say that this has been caused by the lamentable mortality amongst our professional brethren, who with so much courage devoted their energies, and too often forfeited their lives in the discharge of their arduous duties. This is a subject to which frequent reference has been made in

36. Was there anything remarkable in those primary cases which were subsequently followed by relapse?

37. Did you observe any remarkable modifications of the symptoms in individuals of the same family placed under similar circumstances?

38. In members of the same family, who had contracted fever from the same cause, did you observe much diversity of symptoms?

39. What were the usual sequelæ of the fever?

40. What treatment did you usually find most serviceable?

41. Were wine and other stimulants used in the treatment of this fever?

42. Was bleeding, local or general, resorted to; and if so, please state what was the result, and in what cases employed?

43. Was mercury used; and if so, in what cases, and at what stages?

44. Was opium employed; if so, at what time, and in what cases?

the pages of this Journal, and which we now notice only to account for what might otherwise appear a deficiency in our Report.

We shall now proceed to give the results afforded by a careful tabulation of the various answers we have received, introducing lengthened extracts from the valuable reports, either in the details of counties, or in the summary of the provinces, wherever we consider that our doing so will render the information required more satisfactory. Commencing with the province of Munster, and adopting the classification of the queries already given, we shall take up each county in detail, afterwards give a comparative view of the epidemic as it occurred in each provincial district, and conclude with a general review of the entire subject(*a*).

MUNSTER.

First Class of Queries.—The Outbreak and Antecedents of the Epidemic.

Cork.—The report as to the state of health, previous to the outbreak of the epidemic, from all parts of this county, is that it was healthy, except odd cases of diarrhœa in Innishannon(*b*). The date of the commencement was in February, 1847, except in Mitchelstown(*c*), where it began in November, 1846. It attacked equally those in good and bad health, but in some instances, as in Innishannon and in Cove(*d*), many in the best health, while in Mitchelstown the majority had previously suffered from privation. Both the poor and rich appear to have

(*a*) The Editor here begs to express his obligations to his friends Drs. Neligan and Aldridge, for the very great assistance they have kindly given him in drawing up this report. Indeed, without their aid he could not, owing to severe illness, have been able to publish any portion of it in the present Number of the Journal. He also begs to thank his many country friends who have so kindly assisted him in collecting the necessary information. In every instance the authority upon which the Report is founded is given either with the letterpress or as foot-notes to the pages.

(*b*) Dr. Corbet. (*c*) Dr. Phelan. (*d*) Dr. Orpen.

been nearly equally affected ; the lower classes in the large towns being chiefly attacked. We have only one locality in which the disease appears to have set in after recovery from the effects of starvation, namely, Bantry(*a*). The sudden change of diet appeared in some localities to produce diarrhœa and dysentery. Neither purpura nor scurvy appear to have preceded the fever in this country(*b*). An epizootic disease prevailed amongst cattle, previous to and at the same time as the fever epidemic. Swine are reported to be the animals principally affected in the neighbourhood of Innishannon. The disease appears to have been generally the ordinary pleuro-pneumonia ; but we are expressly told that in the neighbourhood of Bantry cow-pock extensively attacked the black cattle. Young persons appear to have been more liable to the fever than the aged, as it principally attacked individuals between 15 and 35. The average age of those attacked in Cove was 19; the range from 3 to 64. In Mitchelstown, from March to July, 1847, males were principally the subjects of the disease ; from July to February, 1848, females were the most numerous; and from February to July, 1848, males again; while in the other districts of the county it would seem that males were principally attacked. The proportion of cases to the population was in Cove 1 in 150.

Tipperary.—The disease was very prevalent throughout the county of Tipperary. In the town of Clonmel(*c*) the average number of cases in the Fever Hospital (75) rose to 518 in June, 1847. In Cloughjordan(*d*) the fever cases doubled in 1846, and again doubled in 1847. Previous to the outbreak influenza was very general in Nenagh, and measles prevailed in Cloughjordan. The disease commenced in Clonmel in Novem-

(*a*) Dr. Tisdall.

(*b*) For the details of the epidemic of scurvy and purpura, we beg to refer our readers to the late Dr. Curran's observations on that subject, published in the Number of this Journal for August, 1847.

(*c*) Drs. Sargint and Dowesly. (*d*) Dr. Purefoy.

ber, 1846, and throughout the rest of the county in Spring, 1847. In Nenagh(*a*) the health of the district was generally good previously ; but elsewhere people were suffering from the effects of want of food. In country districts the poorer class of labourers and the small farmers were those principally affected. It would appear that the fever often set in after recovery from the effects of starvation, and the scarcity and sudden alteration of the people's food, manifestly, it is stated, tended to produce disease. In Nenagh purpura was a frequent antecedent. Inflammatory diseases prevailed amongst cattle, in some parts previous to, and in others simultaneously with, the epidemic. In Cloughjordan young persons and females were more liable to be attacked with fever; while, in the neighbouring district of Nenagh, the disease most frequently commenced in the old, and often spread from them through the other members of their families.

Limerick.—In the county of Limerick the fever broke out in April, 1847, the health of the population having been previously bad; the sickly and the poorest classes were in general the first affected. The disease very frequently set in after recovery from the effects of starvation ; it was not preceded by scurvy or purpura. Young persons and females were most liable to the disease: in the district of Bruff(*b*) those from 5 to 25 were chiefly affected. In the Cappamore(*c*) Fever Hospital, which was opened the 8th of July, 1847, 1151 cases were admitted up to the 10th of August, 1848.

Kerry.—The general state of health was good previous to the breaking out of the epidemic, which commenced in the beginning of 1847. It first attacked the most destitute class, although the individuals affected were previously in good general health; spreading from them to other classes in society. Individuals of all ages were attacked, but males were more liable to the disease than females. Previously to the outbreak

(*a*) Dr. Cahalan. (*b*) Dr. Fitzgerald. (*c*) Dr. Arthur.

of the epidemic a peculiar disease of the feet was common amongst swine.

Clare.—The health of the district had been generally good until the population began to suffer from want of good food, owing to the potato disease, from which period cases of fever were not infrequent in the remote west of the county(*a*); while in the neighbourhood of Limerick and higher up on the bank of the Shannon, those first attacked with the disease were in the enjoyment of good health(*b*). It does not appear that scurvy or purpura preceded the outbreak of fever; but various epizootic affections were prevalent amongst cattle. Generally whole families, of all ages, were attacked, and males were not more liable to the disease than females. In Clonlara the proportion of cases to the population of the district was something about 20 per cent.

For the county of *Waterford* we refer our readers to the reports of Drs. Carroll and Cavet in the summary of this province.

Second Class of Queries.—The Character and Symptoms of the Disease.

Cork.—As regards the much-disputed question of contagion, difference of opinion, of course, exists. In this county the replies, in most instances, are conclusive as to the disease possessing this character; but in two reports from the city of Cork it is stated, in one that there is no *satisfactory* instance(*c*), and in the other, that the evidence is doubtful(*d*). Fever appears to have affected differently on the various classes of society; the head having been chiefly affected among the higher orders, while the lower exhibited more the symptoms of debility; but in the neighbourhood of Bantry no difference in this respect was observed. In those cases of fever which followed on starvation the type was, as might be expected, of a low nature, much de-

(*a*) Dr. Tuite.
(*b*) Dr. Kidd.
(*c*) Dr. Townsend.
(*d*) Drs. Armstong and Flynn.

bility being present from the outset. On this subject Dr. Phelan writes, that they generally assumed the low maculated type, requiring stimulants from the commencement; while in cases not so circumstanced the character was more inflammatory, with chest affections. The typhoid symptoms generally followed the starvation state early, the average being about the fifth day. The only important difference of character between this fever and former epidemics, noted in the replies from this county, was the great tendency to relapse. Purpuric spots do not appear to have been usually common ; out of 747 cases Dr. Phelan only met with forty-three. Well-marked crisis was not more frequent than ordinary ; according to some, not as constant as in previous epidemics. Diaphoresis seems to have been the most usual critical phenomenon.

Tipperary.—The reports from this county uniformly agree in containing evidence as to the contagious nature of the disease. Gastro-enteric complications appear to have been most common among the lower class, while inflammatory or typhoid fever, according to Dr. Purefoy's observations, prevailed among the upper. In those which more immediately followed upon starvation, exhaustion and dysenteric complications were manifest, typhoid symptoms generally following early on the starvation state. The tendency to relapse, and derangement of the digestive organs, especially frequent diarrhœa, were the chief characteristic differences between this and previous epidemics. In the Clonmel Fever Hospital two especial peculiarities were noticed. " First, the *blue nose,*—the nose of the patient becoming first pale and glossy, and then turning rapidly to a greenish blue, as if going into mortification, or having been frost-bitten. All these cases, except one, proved fatal. Indeed," say our informants, " when we saw this symptom we gave up all hope, though of course we did not neglect the patient. The second peculiarity was, that in some few cases the patients appeared on the mending hand, were able to eat a little, talked, and expressed themselves altogether better, and perhaps in a few

minutes would be found by the nurses *dead*"(*a*). In some parts of the county, purpuric spots are reported to have been common, but in others they were not remarkably so. In the Clonmel Hospital crisis was not well marked, but in the northern part of the county it is stated to have been very frequent. The most usual critical phenomena were diaphoresis, increased action of the kidneys, diarrhœa or vomiting, and occasionally epistaxis.

Limerick.—The evidences of contagion in this county are stated to have been of a very doubtful character. Gastric symptoms appear to have predominated among the lower classes. In those cases which followed starvation the fever was of a low character, generally accompanied by symptoms of gastro-enteric derangement; while in those not so circumstanced it was of a more inflammatory form. Typhoid symptoms followed the starvation state in from five to eight days. "The difference," says Dr. Arthur, "which I have found to exist between this fever and former epidemics was the shortness of its duration, and its extreme tendency to relapse. In relapsed cases the attack was very mild in its commencement, and seldom lasted longer than from four to six days, whilst the relapsed stage was long—from ten to fourteen days—very severe, attended with great debility and prostration of strength." Purpuric spots were very common in the Cappamore Fever Hospital, but were not met with in the other parts of the county from whence we have received replies. Crisis was common; perspiration, deafness, and epistaxis near the termination of the disease, were the most usual critical phenomena; but in many cases there was no well-marked crisis.

Kerry.—The evidence from this county is in favour of the contagious nature of the disease. According to Dr. Crumpe's report, when the fever directly followed starvation, the symptoms appear to have been mild, convalescence generally setting in in a very few days. In Tralee the chief difference noticed

(*a*) Drs. Sargint and Dowesly.

between this and previous epidemics was the prevalence of diarrhœa as a sequela. Crisis was common, showing itself by sudden abatement of pulse, coolness of skin, clean tongue, and a craving for food.

Clare.—The replies from this county agree as to the disease being contagious. In the higher orders the fever assumed a worse type, and was more fatal; while in the lower, gastro-intestinal complications were frequent, dysentery usually following the fever. Dr. Tuite, writing from the extreme west of the county, states that he did not meet with a single case immediately consequent on starvation. Those cases that occurred in the neighbourhood of Clonlara are reported as having proved decidedly less fatal than others not so circumstanced. The tendency to relapse, and also its shorter duration, was noted here, as elsewhere, as characteristic of the late epidemic. Purpuric spots were rarely observed; crisis was common: Dr. Tuite states that he looked for a particular day, and was rarely deceived. The most usual critical phenomenon was perspiration.

Third Class of Queries.—Complications.

Cork.—Dysentery chiefly followed, sometimes preceded, but rarely accompanied the fever, except in Bantry, where it was frequent in all stages. Abdominal and cerebral complications appear to have been most frequently met with, except in Mitchelstown, where the lungs were most generally engaged, and purpura and anasarca frequently appeared. Relapses were most frequent, but not among the highest classes, in the city of Cork(*a*), nor among any classes in Innishannon. The symptoms appear to have been more severe in the relapses than in the primary disease, more especially when the latter was slight; and in Cove the relapse was frequently accompanied by diarrhœa and dysentery. In Mitchelstown, petechiæ frequently appeared in relapse, when they did not occur in the pri-

(*a*) Dr. Townsend.

mary fever; but this was not remarked elsewhere. No morbid phenomena of the heart are reported as having been present. Epistaxis was rather frequent, except in Cove, where it was only remarked in a single instance. It usually occurred in the first stage, and is remarked by Drs. Armstrong and Flynn as being always a favourable symptom, and sometimes critical. Enlargement of the spleen was not observed in any instance. Bed-sores were very infrequent in the county; in the city, where they were rather frequent, they were of the common kind.

Tipperary.—Dysentery most frequently followed, but at some times preceded or accompanied the disease. The digestive organs were almost universally engaged; next to them the lungs, and next the brain; but in Dr. Purefoy's report, he states that there was a remarkable immunity from the two latter. Relapses were very frequent, especially in cases where the primary stage was of short duration. In Roscrea, the disease was chiefly characterized by debility, frequent relapses, diarrhœa, and dysentery(*a*). The stage of relapse was usually much more severe than the primary disease, and, in the Clonmel Fever Hospital, generally accompanied by diarrhœa or dysentery, petechiæ also appearing when they were not present in the primary fever. No morbid phenomena of the heart or spleen were noticed; but Dr. Cahalan observes that "he found Dr. Stokes' suggestion with regard to the action of the heart, as a guide for the use of wine, of great value." Epistaxis was not frequent; when it occurred, it was usually looked upon as a critical phenomenon. Bed-sores very rarely occurred.

Kerry.—Dysentery frequently followed on fever of a light character. Relapses occurred very frequently, and the relapsed stage was characterized by a tendency to diarrhœa. No morbid phenomena of the heart were observed, nor was any enlargement of the spleen noticed. Epistaxis was very rare; and

(*a*) Dr. Kingsley.

bed-sores were only noticed in a few cases, where they assumed the appearance of local sloughing.

Limerick.—Dysentery frequently accompanied or followed the fever, but did not often precede it, except in the neighbourhood of Bruff, where it is said not to have occurred. In some districts, hepatic and gastro-enteric complications were most frequently observed, while in others chest affections were very general. Relapses were very common, in which the disease, according to Dr. Fitzgerald, presented the bilious-remittent character. Petechiæ sometimes appeared in relapse, when they did not occur in the primary fever. The state of the heart does not appear to have been particularly observed. Epistaxis was not common. Dr. Arthur reports that enlargement of the spleen occurred subsequent to, but rarely during fever. Bed-sores were very infrequent.

Clare—In this county dysentery almost invariably followed, and sometimes accompanied, but did not precede the fever. Head symptoms, anasarca, and dysentery were the usual complications. Relapses were very frequent, relapsed cases being usually more tedious, and followed by dropsy or dysenteric affections. Petechiæ rarely appeared in relapses when they did not occur in the primary fever. As regards the heart, Dr. Kidd states that *bruits* indicative of anemia were often heard by him; palpitations were noticed by others. Epistaxis occurred in only a very few instances; and bed-sores were very infrequent. Enlargement of the spleen is stated to have been observed in some few cases.

Fourth Class of Queries,—including the Mortality and Morbid Appearances.

Cork.—In Cove the mortality in the upper and middle classes was about one in every fifty cases; in Mitchelstown, three per cent. in the lower, and about six in the higher classes. In Innishannon, the mortality amongst the higher and middle classes was very much greater than amongst the poor, in

the ratio of about 16 to 1. We have no account of the proportionate mortality in the various classes attacked in the city of Cork. In most districts, it was greater among those treated in their own houses than in hospital, but in some places hospitals exhibited a larger proportion of deaths. In towns and densely populated districts, the deaths were more numerous than in the thinly-populated country parts; "but some of the thinly-peopled mountain districts had suffered fearfully before hospital accommodation was provided for them"(*a*). *Post mortem* examinations were exceedingly rare in this part of the country. Dr. Orpen, of Cove, met with congestion of the brain and ulceration of the bowels in those cases which he examined.

Tipperary.—In this county, except in Clonmel, the report states that the mortality was higher amongst the destitute poor; in Nenagh amounting to one in nine in that class, while it was only one in twenty-five amongst the better. The mortality was greater among the poor, when treated in their own cabins, than in hospitals. The mortality does not seem to have been generally greater in the towns and densely-populated districts than in the thinly-populated tracts of country. *Post mortem* examinations were allowed in only very few places. In Cloughjordan, all cases examined exhibited traces of gastro-intestinal irritation. Dr. Purefoy, of that place, writes as follows:

"Stomach.—In the subjects examined the lining membrane of the stomach was generally pale, and softer than in the healthy state. Occasionally a few vascular spots appeared in the lesser curvature, and towards the pyloric orifice, and in a few cases there were found distinct traces of subacute inflammation of the gastric mucous membrane. In some cases of consecutive purpura ecchymosed spots of this membrane existed, exactly similar to those found upon the skin, appearing either in the stomach alone, or throughout the intestinal mucous membrane. The

(*a*) Dr. Phelan, Mitchelstown.

lining membrane of the stomach was in many instances tinged by a bright orange-coloured bile, which apparently regurgitated from the duodenum. Rarely the contained fluid was found to be of a dark blackish colour.

"Liver.—The liver was seldom found seriously diseased; congestion, or slight softening of structure, were the morbid states most frequently observed; but in one case of confirmed drunken habit this organ was extremely atrophied, being reduced to about the size of a man's fist; its structure so soft and friable as to be readily reduced to a pulp under the fingers, whilst the gall-bladder was so reduced in size as to be little larger than the top of the thumb, scarcely raised above the surface of the liver, and containing merely a few drops of deep orange-red-coloured fluid. This patient died of dysentery.

"Intestines.—A great portion of the mucous membrane of the small intestines was thickly coated by a bright orange-coloured secretion of stained mucus, which was most abundant in the duodenum. In this portion of the intestine either inflammatory congestion or acute vascularity very frequently existed, being most remarkable in the folds of mucous membrane which covered the valvulæ conniventes, these folds appearing as so many deep red-coloured lines passed around the inner surface of the duodenum. Inflammation or congestion of the mucous membrane was of frequent occurrence in the lower third of the ileum, but ulceration was rarely found, except in cases of dysentery, and then the ulcerations were chiefly seated in the mucous membrane of the rectum. In protracted cases of dysentery this membrane has been found literally honeycombed by an innumerable number of ulcers, many being very minute, and a few as large as a four-penny piece, and so deep in some instances as to have completely laid bare the serous covering of the bowel. When diarrhœa of long continuance has terminated fatally, the whole tract of the intestines has been found unusually pale, thin, and attenuated. Peritonitis, or effusions of serum or lymph into the abdominal cavity, were not observed.

"As the history, symptoms, and progress of the disease, taken in connexion with the *post mortem* appearances, proved that the abdominal viscera were the parts essentially engaged in the disease, the other cavities of the body were not examined"

Limerick.—In this county the mortality appears generally to have been very small, but was greater amongst the rich in some districts. Generally speaking, the deaths were more numerous among the cases treated in hospital, but in some parts, where the patients remained in their own houses, the mortality was greater. The mortality was always much higher in the densely-populated districts than where the population was thinly scattered over the country. There were no *post mortem* examinations made that we know of.

Kerry.—The mortality was great in all parts of this county, both in the densely and also in the thinly populated parts of it. Dr. Crumpe states that the only remarkable *post mortem* appearance he saw was minute aphthous ulcers of the bowels.

Clare.—In the west of this county, at one period the lower orders suffered most, but in the neighbourhood of Clonlara, and also in Ennis, the mortality was greater in the higher classes It seems, also, to have been much less amongst those attended in their own houses than those treated in hospitals. Towns and densely-populated districts exhibited the greatest amount of mortality. There were no *post mortem* examinations recorded in our returns from this district.

Fifth, sixth, and seventh Class of Queries,—relating to the Duration of the Disease, Modifications, and Sequelæ.

Cork.—The duration of the disease varied from eight or ten days, or less when relapse occurred, to about three weeks. Convalescence was generally protracted, sometimes lasting for many weeks. In Cove, however, the duration was from a week to ten or twelve days. Nothing remarkable has been noticed in any of the reports as regards the primary fever, which was

subsequently followed by relapse, except from Mitchelstown, whence Dr. Phelan writes: "That when the primary case was slight, the relapse was severe and continued; and *vice versa*, when the primary was severe, the relapse was slight, and not near so frequent." No remarkable modifications of the symptoms were observed in individuals of the same family, placed under similar circumstances, nor any diversity of symptoms in members of the same family, when fever was contracted from the same cause. But Dr. Orpen, of Cove, states that children were more liable to bronchial affections than their parents. In the city of Cork, "erysipelas of the head and face, rheumatism, and abscesses with extensive formation of matter burrowing over the cartilages of the ribs, pointing over the sternum generally with many openings, and not at all amenable to treatment"(*a*), were the usual sequelæ. Gastric derangements, dysentery, and anasarca both local and general, were more frequently met with in the country districts.

Tipperary.—The usual duration of the disease in this county was from one to three weeks, and the convalescence generally lasted from fourteen to twenty-one days; occasionally only five days. Those primary cases that were followed by relapse were usually milder than others. Dr. Cahalan remarks that those primary cases in which gastro-enterite or enteritis was present, required more watching to prevent a relapse. No modifications of the symptoms in individuals of the same family, similarly placed, were noticed, except by Dr. Purefoy, who constantly observed such. According to his return, members of the same family, who contracted fever from the same cause, often presented much diversity of symptoms, especially in the local complications. The most usual sequelæ were dysentery or diarrhœa, anasarca, and rheumatic pains. "The dysentery was often fatal in convalescence"(*b*).

(*a*) Drs. Armstrong and Flynn. (*b*) Dr. Kingsley, Roscrea.

Limerick.—The usual duration of the disease was from a fortnight to three weeks, and convalescence was generally protracted. Those primary cases which were subsequently followed by relapse were usually mild, and of short duration, " and presented more of the bilious remittent character"(*a*). In some instances the symptoms were different in members of the same family who contracted fever from the same cause. Dysentery and great debility were the usual sequelæ.

Kerry.—In this county no peculiarity is stated with respect to the duration of the disease or the length of convalescence. Those primary cases which were subsequently followed by relapse generally lasted only for a very few days, and the patients were affected with a craving for food amounting to *bulimia*(*b*). Great debility generally followed recovery from the disease.

Clare.—The average duration of the fever was about fifteen days; sometimes extending to three weeks. The period of convalescence varied from a fortnight to six weeks. Those individuals in whom relapse occurred were generally badly fed; " of dirty habits"(*c*); and, according to one return, " remained too long in hospital exposed to contagion"(*d*). " Female members of the same family, though they had contracted fever from the same cause, generally suffered less than males." Anasarca, dysentery or diarrhœa, and general debility, were the usual sequelæ.

Eighth Class of Queries,—referring to the Treatment.

Cork.—The general result of our inquiries, under this head, show that a sthenic plan of treatment was adopted, depleting remedies of any sort not being admissible. In the early stage of the disease mild alterative aperients and diaphoretics were occasionally used, but in general very little medicine seems to have been employed. Dr. Corbet, of Inishannon, states that

(*a*) Dr. Fitzgerald, Bruff.
(*b*) Dr. Crumpe, Tralee.
(*c*) Dr. Tuite, Kilkee.
(*d*) Dr. Kidd, Clonlara.

he placed most reliance "*in tepid sponging, judicious nourishment, and good nursetenders above all.*" Wine and other stimulants were *largely* and very generally used. Bleeding, either general or local, was very rarely resorted to; the former in not more than one or two instances ; the latter occasionally with good effect in local congestions ; but Dr. Townsend, of Cork, states that, in his practice, " moderate local bleeding was almost invariably called for from the beginning." Mercury was sometimes used as an alterative, or with chalk and Dover's Powder to check diarrhœa, but never with the intention of producing its specific action. Towards the termination of the disease opium was given when watchfulness or diarrhœa prevailed, and with advantage.

Tipperary.—The stimulant plan of treatment was freely adopted in Clonmel, and with excellent effect. Elsewhere in the county a mild alterative plan was generally found more serviceable. Wine and other stimulants were generally used. General bleeding was seldom employed, except by Dr. Purefoy, from whose lengthened report we quote as follows: "Bleeding, either local or general, but especially the latter, I have always found to be one of the most efficient and valuable remedies employed in the treatment of fever. During the spring and summer months of the years 1847, when the epidemic had not been of many months' continuance in my district, bleeding was employed in about ten per cent. of the patients admitted to hospital, and with decidedly good effects. The general indications were, fever with inflammatory symptoms in its early stage ; the disease occurring in young and healthy subjects ; the pulse being full, frequent, and of good strength. However, as the season advanced, and the disease continued to spread, these indications became less frequent, so that during the winter of 1847, and the first eight months of 1848, venesection has not been employed in one case in a hundred. Local bleeding has been used occasionally for the relief of urgent symptoms of

the head or chest." Mercurials were used with advantage, but not to produce their specific effect. Opium was chiefly employed in diarrhœa and dysentery.

Limerick.—Mild alterative treatment seems to have been generally used with advantage in the treatment of the fever in this county. Dr. Fitzgerald generally commenced with the administration of gentle emetics. Wine and other stimulants were very much used. Dr. Arthur used general bleeding in a few plethoric cases, seemingly with good effect, but states that, from his experience, he placed no confidence in depletion in the present epidemic. Local bleeding by leeches was the only form of depletion employed in the Bruff Fever Hospital, and that only in some few instances. Mercury was only used as an alterative. Opium was employed with benefit, especially when a tendency to diarrhœa existed.

Kerry.—An expectant plan of treatment, with especial attention to diet, was used in this county in the treatment of the epidemic. Wine and broth were early employed, and found most serviceable. General bleeding does not appear to have been employed, and only leeching to the head, when the brain was much engaged. Opium and mercury combined were found of advantage when diarrhœa or dysentery followed fever.

Clare.—From the returns we learn that in this county also but little faith was placed in medicine in the treatment of the epidemic. Wine, broth, and other nourishment, with good ventilation, were chiefly depended on. Preparations of mercury were only used as alteratives, and opium very seldom employed, except where diarrhœa or dysentery prevailed.

Summary of Reports not employed in the foregoing Extracts.

Dr. Burke, of Kilfinane, a small town in the south-east of the county of Limerick, has afforded us the following report:

"From the 1st of August to the 27th of October, 1847, I admitted ninety-seven patients labouring under fever; the greatest number of admissions in one week being sixteen. Five died in

the hospital during that time; of these, however, only one died of the simple epidemic, the complications in the other fatal cases being as follows: The first had been long labouring under chronic bronchitis, from the effects of which he died on the fourth day of his admission. The second had all the symptoms of malignant typhus when she came to hospital, and died of that disease three days after she was admitted. The third person was a woman aged 46, of broken-down constitution, affected with recto-vaginal fistula, dysentery, and ulceration of the genital organs. The fourth died of severe pneumonia, the consequence of cold caught by imprudent exposure when he was perfectly convalescent and walking about. The fifth died of the epidemic in a pure uncomplicated form; he lived eight days after admission, and during almost the whole of that period he was bathed in profuse perspiration. Stimulants were unavailing to control the disease, although early and liberally applied. He gradually sank, and died perfectly sensible up to within three hours of his death."

Dr. Burke has here supplied us with a table showing the number of days each person spent in hospital, together with the ages and sexes of the patients, from which we learn that the average number of days spent by each patient was about thirty; and of ninety-seven patients, fifty-three males and forty-four females, the ages were as follows:—Under ten, 9; from nine to twenty, 45; from twenty to forty, 26; and over that age, 17. Of this number eight had been ill but one day before admission; twenty-eight, ten days; twenty-eight, three days; twenty-three, four days; five, from five to six days; and five, from seven to fifteen days. He then continues,—

"The great liability to relapse in the present epidemic must, I am sure, have struck every practitioner who has had an opportunity of observing its progress. Probably, the difficulty of complete separation of the convalescents from those in whom the disease was in full vigour, owing to the necessarily hurried and imperfect nature of the hospital accommoda-

tion, may have had some influence in causing such to be the case. I observed that those who got over their first attack most rapidly, and with least suffering, were invariably the persons most liable to relapse. Of my ninety-seven patients, twenty-eight relapsed; and of these twenty-eight, five underwent a third attack of the disease. Four of the entire number, after having been dismissed cured, were re-admitted at various intervals after going away; and I found in each of the four cases that the second invasion of the disease was more severe than the first attack. Besides the ordinary familiar symptoms of the disease,—the shivering, the headach, the pains in the back and bones, the pulse ranging from 90 to 100, and the white, thickly-coated tongue,—I found in several of the cases a considerable degree of epigastric tenderness, and a feeling of fulness and loathing about the stomach, which led several to beg I would give them an emetic, at all times a favourite remedy with the country folk. The thirst I always found a prominent and distressing symptom. In twelve cases I saw nasal hemorrhage to some extent, and in ten rather smart diarrhœa. Whenever there was a well-marked critical period, which did not occur very frequently, sweating was its most prominent attendant. Four of the cases assumed well-marked typhoid symptoms during the progress of the disease.

"In no one case did I find it necessary to bleed, and very few indeed bore even the moderate exhibition of tartar emetic, unless given with great caution. Mercury I thought in any shape decidedly injurious. The best purgative I found to be castor oil, and latterly I administered none other. I found the diarrhœa very amenable to the preparations of kino and catechu, and these I did not administer unless the symptoms seemed inclined to become obstinate. The pain of head yielded to simple cold water and vinegar; and I found blistering the best remedy for the epigastric tenderness. In the very great majority of the cases under my care, it was necessary to begin the use of nourishment and stimuli at a very early pe-

riod. I found a valuable adjunct in the decoction of polygala senega and carbonate of ammonia, when the bronchial tubes became engaged in the progress of the disease."

In the city of Limerick Dr. Griffin says, that "the disease was at its maximum intensity in April, 1847; that relapses were very frequent; that, in the commencement of the epidemic, the fever was of a mild character; but towards its termination it assumed a typhus type. Diarrhœa and dysentery were very frequent complications, and, in local determinations, bleeding and cupping were attended with the best effects."

In addition to the information already given under the various headings of the county Kerry, Dr. Crumpe has forwarded us the following report upon the peculiarities which the fever exhibited in the Gaol of Tralee. "Circumstanced as our gaol is, built on a flat from which there is no fall, and the hospital small and ill ventilated, the sewers and necessaries become quickly choked up. The crowds of poor, starved wretches, hurried in droves to gaol for some petty thefts, generally perpetrated for the purpose of being committed to gaol to be saved from death by starvation, were quickly taken off by death from disease. These also quickly fell victims to fever, and from them it spread among the healthy classes in the gaol, who heretofore enjoyed good health, and never suffered from starvation. The hospital soon became over-crowded, though my call for more accommodation was urgent long before it was attended to.

"In this horrid den those labouring under local disease, those ill from fever, those dying, and the dead from fever and dysentery, were promiscuously stretched together. So insufferable was the atmosphere of the place, so morbidly fetid and laden with noxious miasma, notwithstanding constant fumigation with chloride of lime, that on the door being opened I was uniformly seized, on entering, with most violent retching; and it is singular that I should be so affected, who dissected so much, have opened so many bodies, performed so

many operations, and see often such forms of loathsome disease: yet the fact is so. Such was the polluted and contagious state of the air, that the nursetenders were quickly attacked, and some died. In this place my visits were as short and hurried as could be; I was forcibly driven back by the smell. The mortality was enormous, deaths often taking place a few hours after admission; but this occurred in the most exhausted and worn-down subjects. A few cases were seized with vomiting, throwing up large quantities of black stuff from the stomach; some few were jaundiced all over. But, generally speaking, the form was mild, commencing with chills and wearisomeness, followed by heat of surface, slight headach, quick pulse, white tongue, and loathing of food. By confinement to bed, and simple drinks, these symptoms subsided, in a very few days, sometimes from three to five, often without any medicine, being succeeded by a ravenous appetite, which it was most difficult to regulate and control. Relapses and re-relapses were most frequent; diarrhœa generally setting in, which no medicine or treatment could check. Had these cases occurred among previously healthy, well-fed subjects, in private life, where proper ventilation could be preserved, and airy apartments procured, recoveries would be more frequent. Many of the gaol guards were attacked; none of these died in gaol; they were well fed, and had better apartments.

"In the female department cases of fever and dysentery were comparatively few, more cleanliness of person being enforced among them, and they were not so crowded together.

"So foul was the atmosphere, so cadaverous was the smell, that I could not make *post mortem* examinations, nor was there any accommodation to do so, though the bodies were numerous, and often no claimants for them. From one *post mortem* which I saw in the military barrack, and from the symptoms and appearances,—slight, minute ulcers on the mucous coat of intestines,—I am convinced the same appearance would be found in all those fatal cases where diarrhœa succeeded fever. Strong,

healthy, robust persons, with local disease, who would insist on going to hospital *contrary to all advice*, were quickly attacked by the same fever which their fellow-patients had, and soon fell victims. I do not know any circumstance which is a stronger proof of the contagiousness of the disease than the nursetenders being attacked."

The annexed quotations are from the extended report of Dr. Cullinan, of Ennis.

"The epidemic fever which has lately desolated this country has prevailed extensively in this neighbourhood, and I regret to add that it continues still (November, 1848), in many places, but little abated in its extent or virulence.

"It appeared rather suddenly at the end of the year 1846, and, with a steady and regular progression, attained its greatest intensity in a period of about six months, and then gradually declined up to the early part of the present year (1848), when its progress was again marked by a very evident increase. The number of admissions into the County Clare Fever Hospital, in November, 1846, was only ninety-three; it rose to 224 in December, and in the following June it amounted to 757, its maximum in any single month. The admissions fell by a steady and rather regular rate to 210, in February, 1848, and again, by a rapidly progressive increase, attained to 705 in the following May.

"Before the occurrence of the epidemic fever the sanitary state of the district was decidedly below par. Scorbutic diseases obtained widely among the poor, and the public mind generally was greatly depressed. The scurvy had followed in the train of the *epiphytic*(*a*) by which the potato had been attacked, and, I believe, was partly caused by an absolute

(*a*) "There is obviously a defect in our nomenclature which should be supplied. *Epiphytic* (επι and φυτον) is the analogue of *epidemic* and *epizootic*, and is now equally useful and necessary as the germane generic terms. Indeed the term *epidemic* is sometimes applied to the disease affecting the potato, but it is obviously a misnomer."

deficiency of food, and in a great degree, also, by the sameness and bad quality of the food substituted for the potato, together with the exposure to cold and wet, to which the peasantry were subjected in an unusual degree.

" The epidemic affected chiefly the peasantry and the poor, which recently have become nearly convertible terms. Very many clergymen, physicians, and gentlemen connected with relief committees and the administration of the poor law, have been also attacked; but generally the other classes of the gentry have enjoyed, comparatively, a great immunity from fever.

" The average mortality in the County Clare Fever Hospital, for ten years preceding the epidemic, was, I think, 1 in $13\frac{3}{4}$, or about seven per cent. in round numbers; in 1846, the mortality was 1 in $12\frac{1}{2}$; in 1847, 1 in $5\frac{3}{4}$; and in 1848 (up to the present), 1 in $5\frac{1}{2}$. I think that the mortality was much *less* in those attended in their houses than in hospital. Among the clergy, physicians, and other gentry, the mortality was about twenty-five per cent, as far as I can judge by my own experience. I may observe, generally, that the poor, before the access of fever, were suffering from the effects of bad diet, and other privations; and that the gentry who were attacked were depressed by excessive bodily fatigue and great mental anxiety.

" With regard to the question of contagion I feel rather disinclined to express my opinion, as the views which I entertain are different from those advanced by the highest authorities in the profession. I find it difficult to convince myself by fair logical proofs that the spread of fever has been materially influenced by contagion. We have here a special disease, which is frequently generated *de novo*, and can, at any time, be produced by several causes, operating singly or concurrently; and it requires strong positive evidence, such as has but rarely fallen under my observation, to satisfy me that any special case of fever has its origin in contagion. On the other hand, the negative evidence, the evidence against the influence

of contagion to any considerable extent, is very full, and as convincing as such evidence can ever be considered. I can state as a fact, that, during the prevalence of this epidemic, I have seen over thirty of the gentry attacked with severe typhus fever and in no case has a second member of the family become affected, although in no instance was there a system of separation or seclusion resorted to. On the contrary, in every one of those cases, different members of the several families were the most constant and indefatigable attendants of the sick. I have in no case seen one of them attacked with fever !

" I think that the food of the peasantry, since the failure of the potato, is not only insufficient in quantity, but that it does not include sufficient variety to supply all the elements of the human body in the quantity and proportions necessary for maintaining health, certain elements being in proportional excess, while others are deficient, and others altogether wanting. I am sensibly persuaded that the effluvium from the body of a person living exclusively on Indian corn differs widely from that of one fed upon potatoes or a mixed diet. I have long been familiar with the odour of the persons and clothes of the rural poor of this country, and I can confidently state that that has been succeeded by something of a different kind and far more offensive. Even in the open air, when passing through a crowd of poor persons congregated at a relief depot or a workhouse door, I can perceive the air sensibly vitiated by the emanations from their bodies; and these effluvia are far more readily cognizable, more offensive, and more sickening, than the sooty and peat-smoke odours of former times. I believe (indeed I suppose no one will deny) that many persons have been attacked with fever from breathing this vitiated and pestilent air (particularly in close and crowded apartments), although no individual in the crowd may be labouring under the disease ; and therefore I am of opinion that this congregating and crowding together has been one of the most effective means of disseminating typhus fever. I have, however, no doubt that most of

the cases of fever which occurred among professional men and gentry may be readily traced to attendance at relief depots and ministration to the wants of the crowded and sometimes sick poor.

" With regard to the character of the fever itself, it was marked with many peculiarities, some of which I will mention. The fever was generally of a low typhoid type ; this, I think, was its uniform character when professional men and other gentlemen were its subjects. In these cases there were always marked symptoms of profound lesion of the brain and nervous system, excessive debility, great want of rest, delirium, subsultus, frequent respiration without pulmonary disease, and, finally, coma. In almost every case of this class there were maculæ, generally of a dark colour. The action of the heart and pulse was feeble; inflammatory affections of the thoracic and abdominal organs were very rare. The other symptoms were variable. Among the poor there were some cases of the same character as those I have described. Petechiæ were very general—but the most frequent and most fatal complication in this class was dysentery, sometimes preceding, often accompanying, but most frequently following fever. The dysentery was very different from that commonly observed. Its access, for the most part, was quite sudden. A patient apparently convalescent or making favourable progress would have twelve or twenty alvine evacuations in a few hours, consisting of a serous bloody fluid, often without a trace of mucus or fecal matter. This fluid resembled very closely, except in colour, the rice-water evacuations in malignant cholera. Sometimes the redder and more substantial portion would subside, and present the appearance of a dark red, uncoagulated, homogeneous, viscid mass at the bottom of the vessel. The disease was attended with great debility and restlessness, considerable pain, and often tenderness on pressure. It very often terminated in chronic diarrhœa.

" Young persons from 5 to 15 were the most frequently

attacked. I could not observe that the proportion of males and females differed from that which had obtained previously, viz., about fifty-six females and forty-four males in 100.

" Some special and novel facts which seem worthy of record occasionally presented themselves. During the progress of the present epidemic I have noticed for the first time very many cases of tonic spasm, or what Cullen calls "spastic rigidity" of the extensor muscles of the neck occurring in the progress of fever. I presume this state was a symptom of cerebro-spinal arachnitis. I treated them with mercury, local depletion, and counter-irritation, and many of them eventuated favourably. I have at present under my care a young woman, now almost quite free from febrile disease, who, for the last three weeks, has suffered and still suffers from this partial opisthotonos to an extent I never before witnessed.

" There were presented to my observation some remarkable cases of fever, which ran a fatal course in a period of from six to twenty-four hours. They were not cases of cholera of any kind. At first I supposed that their rapid progress was influenced by previous privations, but now I am of opinion that their course and issue were determined by the agency of some potent aerial poison. I remember particularly two such cases, which occurred in the county Clare gaol, where the food was always abundant and wholesome. A young man had eaten his breakfast, and began to complain in two hours afterwards. I found him weak, so that he could not stand without support; his head was giddy, his eyes glassy and without expression, his pupils contracted, and vision dull; his skin was cool; pulse very weak, almost imperceptible; the action of the heart rapid and indistinct; secretion of urine scanty; tongue rather clean, but clammy. He was laid in bed, got some wine and chlorate of potash, and had a sinapism applied to his epigastrium. He sank gradually, and died in eight hours. The second patient died in little more than six hours from the time of his first complaining. His symptoms were very simi-

lar to those detailed. The gaol at this time was greatly crowded, and fever and dysentery were very prevalent and fatal among the prisoners.

"During the epidemic, relapses were frequent, and typhoid symptoms were more developed in the second than in the first attack. Many of these cases were remarkable. For example, a person was attacked with symptoms of active fever, and recovered unexpectedly in two or three days; in a few days afterwards he was attacked again with symptoms of a graver kind, with well-marked cerebral derangement, great prostration of strength, and maculæ generally. These cases were always dangerous and often fatal. They were so common as to arrest the notice of the people, who called the first attack the '*short* sickness.' Such was the case of Dr. Nihil, of Tulla. After an active ephemeral attack he was able to resume the arduous duties of his profession, and in a few days was suddenly struck down with maculated typhus of a most serious character. The case of Dr. Williams, of the Scariff union, was also of this kind. The symptoms of the first attack were very active; his head was shaved, and leeches were applied, and on the following day he was walking about, apparently quite well, and continued so for some days. I predicted to his medical attendant that there would be a relapse in this case; my prediction was verified, and Dr. Williams passed through severe typhus fever.

"During the epidemic, crisis was universal, epistaxis was rare, and I witnessed true purpuric spots only in a few cases. The convalescence was tedious in very many cases.

"With respect to the treatment adopted by me, I have but little to say; it was generally expectant, and, as complications arose, it was suited to the special indications. Mercury was not much employed; it did not seem to exercise any considerable influence on the dysentery, the most frequent complication. General bleeding was but rarely resorted to; I have, however, employed it with advantage in some cases where local inflamma-

tion existed, and also in some other cases without inflammation; and I have not been deterred from doing so by the existence of maculæ. I found it useful in all the cases in which I used it. Local bleeding was frequently employed, for the head chiefly, but not to the extent or with the same benefit as formerly. Opium was used in great quantity, chiefly to relieve diarrhœa and dysentery. I have employed it with excellent effect, combined with acetate of lead, in the treatment of the bloody flux before referred to. I frequently gave sixteen grains of solid opium, with twelve grains of acetate of lead in twenty-four hours, and often without producing sleep, or sensibly alleviating pain. So slight were its effects, that I have sometimes suspected that the opium was not pure: it had, however, been obtained directly from a respectable house in Dublin. Wine and other stimulants were used more liberally than usual, but with much greater reserve, I believe, than is practised in the metropolis. I have employed chlorate of potash freely, and with advantage, as I suppose, where the typhoid type of fever was well marked, and the quality of the blood deteriorated.

"We have at present (November, 1848), 400 patients in the County Clare Fever Hospital, and I fear the epidemic tendency is far from being exhausted.

"With the views I have already expressed, as to the etiology of the epidemic, the proper course for arresting its progress is sufficiently obvious. A better and more varied diet, if possible, should be provided for the poor. Indian corn, on which they must subsist for many months to come, should be carefully analysed, and its elementary composition compared with that of the human body; and those essential elements which are deficient or altogether wanting in Indian corn should be supplied by providing other dietetic articles, specially selected, in which these elements are known to exist; or perhaps, like iron in chlorosis, they may be introduced directly into the system. For instance, let the chemists say whether maize contains nitrogen, sulphur, sodium, phosphorus, &c., or any of them, in sufficient quantity

to supply the wants of the system (regard being had to the general condition and habitudes of the poor), and if so, let them indicate the dietetic articles which may compensate for those deficiences, or let them suggest the most eligible mode of introducing these principles immediately. I could mention many cases of disease in which I have adopted the practice here suggested, and, as I suppose, with advantage."

The following report has been received from Dr. Carroll of Waterford:

"The recent epidemic, which raged with great violence in this city and neighbourhood, commenced in October, 1846. In the early part of that year there was nothing remarkable in the state of the public health, the average number of patients in the hospital not exceeding that of ordinary times. At the period mentioned, however, a gradual increase of admissions was observed, and this continued until the epidemic arrived at its acmé in the summer of 1847, when we had no less than 490 cases together in the hospital and adjoining temporary sheds. I have not the slightest doubt that the fever was highly contagious, indeed I possess undoubted proofs of the fact. A large proportion of the officers of the institution were attacked by it, and I regret to add that several valuable lives were lost, as the following list of deaths will testify: two matrons, two apothecaries, two porters, and one nursetender, besides the physician of the Waterford Dispensary, fell victims to its ravages. I could adduce a vast amount of evidence to the same effect, but the above will suffice.

"The class of persons among whom the disease chiefly prevailed were, at the commencement, the destitute, ill-fed, and ill-clad inhabitants of the lanes and ill-ventilated districts of the city and suburbs; from these it ascended, and more or less pervaded all ranks and classes of society. I cannot state accurately the proportionate mortality which occurred in the respective classes, but I can say in general terms that its fatality increased in the ratio of the rank and respectability of the

individuals attacked; the difference in the symptoms of the disease, as it appeared in the higher and lower classes of society, consisted chiefly in the greater predominance of cerebral complications in the former. In the autumn of 1847 dysentery made its appearance as an epidemic distinct from the fever, though probably owning a similar origin. It presented itself in a very severe form, and was exceedingly fatal. It often attacked our convalescents from fever, and the great majority of those so attacked were carried off.

"The local complications chiefly noticed in this epidemic stood in the following order of frequency: affections of the gastro-intestinal mucous membrane; of the brain; of the lungs and pleura; and erysipelas.

"The greater proportion of those attacked were between the ages of 25 and 45, and females much more frequently than males; the proportion being about five to three. In the great majority of the cases which fell under my observation the usual duration of the disease was from fifteen to twenty-one days: the seventeenth day may be stated as the period at which convalescence was most frequently attained. These cases presented all the features of the ordinary typhus fever of this country, modified in some degree by the previous privations of the sufferers, which caused a much earlier and more profound prostration of strength than I had witnessed in any previous epidemic.

"There was, however, another class of cases quite distinct from these, in character, duration, and the great tendency to relapse by which they were accompanied. In these, fever commenced suddenly, with great violence, and with little premonitory disturbance of the system, and generally terminated on the seventh or ninth day by profuse sweating, which was frequently preceded by rigors. About two-thirds of these cases relapsed. The period of relapse was from the third to the fifth day after convalescence, and there was a singular uniformity in the symptoms which ushered it in. It almost always began

with a rigor which was soon succeeded by vomiting of a greenish yellow fluid, great tenderness in the epigastric and right hypochondriac regions, prostration of strength, and, if the patient was old, or previously enfeebled by disease, collapse and death; if, on the contrary, he was young, and previously healthy, recovery took place on the seventh or ninth day. These cases were never maculated, but sometimes presented purpuric spots; but, in the typhoid cases, maculæ were invariably and extensively present: they generally appeared on the fifth day of the disease in rose-coloured patches, which became darker as the fever advanced.

"I cannot say that crisis was common, except in the seven-day fever, when it invariably occurred by sweating. When it happened in the typhoid cases, it was generally by the supervention of diarrhœa, about the fifteenth day. Profuse perspiration in the latter stages of typhus is a most dangerous symptom, and frequently the harbinger of death. This fact has been noticed by several writers on the disease, and was fully borne out by my experience in the late epidemic.

"Epistaxis was a very frequent occurrence, and often to such an extent as to require the posterior nares to be plugged. It often relieved the head symptoms, and these cases generally did well: the period of its occurrence was between the sixth and tenth day.

"The most frequent sequela of the fever was inflammation and suppuration of the parotid glands, which was the cause of much suffering and protracted convalescence.

"Erysipelas of the head and face was a frequent and formidable complication of the disease, and we were never without a considerable number of cases of it in the wards. It generally made its attack in the latter stage of fever, or during the period of convalescence, terminating in abscesses about the face and scalp.

"With respect to treatment, one leading principle was strictly adhered to in this epidemic, viz., to avoid all heroic remedies

or modes of practice, and to support the strength of the patient from an early period of the disease. As a general rule, very little medicine was prescribed: the bowels were regulated by gentle doses of calomel and rhubarb, or castor oil; but even these were cautiously employed, as there was, in most cases, a tendency to diarrhœa. About the fifth or sixth day, beef-tea was generally added to the ordinary fever diet; and, in many cases, when the patient was weak, a pint of table beer per day was also allowed. This we found a valuable adjunct in supporting the patient's strength, and often superseded the necessity of giving wine or other stimulants. Under this simple treatment, together with attention to ventilation and cleanliness, a large proportion of the ordinary run of cases did well.

"The stimulants used in our hospital were Port wine, and whiskey made into *punch*. In all the heavy cases of typhus these were administered to a greater or less extent; but always with caution, and never in the '*ad libitum*' manner, which I fear is now too fashionable. I feel satisfied that, beyond a certain reasonable amount, they do no good, but, on the contrary, have a tendency to produce or increase dangerous congestion of the brain. While on this subject, I may observe that I have derived much assistance in their exhibition from Dr. Stokes' valuable researches on the condition of the heart in typhus.

"General bleeding I have never once resorted to in this epidemic, but I found the local abstraction of blood by leeches or cupping an invaluable remedy in many of its complications, particularly in the early stages of the disease. When there was much headach, with heat of scalp and throbbing of the temporal vessels, the application of a few leeches, repeated in relays of small numbers, often acted like a charm, and seemed, in many instances, to cut short the attack, or at least to render its subsequent career extremely mild. I have also leeched the head in a more advanced period, with the view of procuring sleep, as suggested by Dr. Corrigan, and often with the desired result.

"In all local inflammatory complications, whether of the chest or abdomen, local depletion was resorted to whenever the general condition of the patient admitted of it. I, however, observed, that loss of blood, even to a small extent, was in general badly borne after the first week, and even then we had frequently to administer wine at the same time.

"I have not used mercury so as to affect the system in any case of simple essential fever, but, when complicated with pneumonia or pleuritis, I have found it of the highest efficacy in controlling those dangerous affections. Calomel and opium was the form selected in those cases, conjoined with local bleeding or blistering, according to circumstances. In the more acute forms of cerebral irritation it was also administered, combined with antimony, until the gums were affected, and with the best results.

"I have employed opium very frequently in the late epidemic, and on no former occasion have I seen it produce such satisfactory effects. I believe there is no procedure in the treatment of fever which requires so much nice discrimination and judgment on the part of the physician as the exhibition of opium. It is playing a hazardous game. It either saves the patient or fearfully increases his danger. In that species of delirium with sleeplessness, which generally occurs between the eighth and twelfth day, resembling in its character delirium tremens, and which, if unrelieved, will often persist for several days and nights, until the patient dies, a pretty full dose of opium acts like a charm, the patient awaking in the morning perfectly composed and tranquil. There are, however, a large number of doubtful cases where the indications are far from being clear; here the difficulty exists.

"I have noticed the following symptoms as favourable to its administration:—a pupil *not contracted;* soft, perspirable skin; abundant secretion of urine; and a compressible pulse. The form I have found most advantageous in the majority of cases was *Dover's Powder*."

Dr. Cavet writes from the same locality:—"I have had the disease twice myself during the year 1848, and out of six months I was attached to the Waterford Fever Hospital, I served only two. The first attack appeared five days after entering on duty at the sheds; but I do not believe it was contracted in them, but in one of the miserable holes I had visited just before. It was attended with yellowness of the skin, hiccup, and vomiting for three days, some of the matters brought up being bluish, and was followed by a short collapse. Recovery was attended by severe neuralgia of the upper extremities.

"The second attack followed in about thirty hours the autopsy of a typhus patient, who had exhibited most marked symptoms of poisoned blood. The skin of my hands was entire at the time, but absorption must have taken place in the left, as the first thing I remember was a large rupia-vesicle at the root of the thumb, and inflammation of the axilla and left side of the thorax, which ended in suppuration.

"This time I had great prostration and furious delirium, and was carried through by the most liberal use of stimulants. Rheumatism followed, and still annoys me in the hands and feet.

"Dr. Thomas Kehoe, Staff Assistant Surgeon, who assisted me in the examination, suffered from similar but milder symptoms."

In addition to the answers to our queries from the Cork district, and which are given in the county details, Dr. Jones Lamprey has just furnished us with the following report of the epidemic as it appeared in the district of Skull and Skibbereen, where both famine and pestilence raged with probably greater fury than in any other part of Ireland:

"Towards the latter end of the month of March, 1847, I was appointed by the Central Board of Health to take charge of the district of East and West Skull, which at that time was suffering under a frightful state of famine, epidemic dysentery, and fever.

"I found the district that was allotted to me very extensive, thickly populated, situated at the extreme south-west promontory of Ireland, within a short distance of Cape Clear, and bordered by a sea-coast marked by numerous islands and a vast variety of natural harbours. The appearance of the country is exceedingly rocky, and contains several large tracts of bog. Very few gentlemen's residences were to be seen, but numerous cabins of the worst description were visible in all directions, especially along the coast.

"The population, previous to the year 1847, amounted to almost 18,000, the great majority of whom were entirely dependent on the local employment of their labour, either as fishermen, miners, or in the cultivation of the potato; these resources having always afforded constant employment, and the rich land and fisheries yielding a plentiful supply of food.

"It would be difficult to state at what particular date the epidemics commenced to be prevalent; but, from the general remarks of the residents, I should say that fever and dysentery were developed towards the latter half of the previous year (1846), as these diseases showed themselves along with the decline of the food of the people, and arrived at their maximum at the time the famine was made complete by the total failure of that year's harvest crop of potatoes. Disease then, in company with famine, spread most rapidly, and carried off numerous victims(*a*), reducing the population so rapidly, that by the close of that same year 2000 had fallen.

"No particular class was exempt from the prevailing fever or dysentery, though, perhaps, if there had been any means of

(*a*) "Frightful and fearful," writes Dr. Traill, February 2, 1847, "is the havock around me. Our medical friend, Dr. Sweetman, a gentleman of unimpeachable veracity, informed me yesterday that if he stated the mortality in my parish (Skull) at an average of thirty-five daily, he would be within the truth. The children in particular, he remarked, were disappearing with awful rapidity. And to this I may add the aged, who, with the young,—neglected, perhaps, amidst the wide-spread destitution,—are almost without exception swollen and ripening for the grave."

making an exact calculation, the number of cases occurring among the independent were proportionably greater than among the poor. This is to be explained by the habit the poor starving creatures had of crowding around the houses of the better classes, seeking relief, and many of the houses of the gentry being even made depots for administering it, their humane families carrying on the office of distribution. From this circumstance many contracted fever or dysentery, and many excellent individuals have fallen victims to their zeal and extraordinary efforts to alleviate the overwhelming distress with which they were surrounded; foremost among whom may be mentioned the Rev. Dr. Traill, whose philanthropic exertions to relieve the stricken people were only checked by the sacrifice of his devoted life.

"I am not aware of disease being prevalent previous to these epidemics of 1847–48, nor had there been any epidemic among the cattle, though, in fact, I saw but few cows, sheep, or poultry in the whole district, which I found to have arisen from the farmers having disposed of them through fear of their being plundered by the starving people.

"Those persons who were suffering from starvation were usually attacked with a lingering form of fever, 'famine fever,' characterized by great prostration, thirst, a dry, chaffy, hot feel of the skin; a weak, feeble pulse; the intellect generally clear; no cerebral, thoracic, or abdominal complications; nor was it defined by periods, stages, or crisis, and terminated in death from inanition alone. It required no medical treatment for its cure, as recovery took place on administering proper nutriment; it was modified according as the individual was able to procure sustenance or not. In many instances anasarca appeared as a sequela, and persons so affected, especially towards the latter stages, exhaled a peculiar fetid, septic odour. This form of fever was not propagated by contagion, but persons so affected, when brought in contact with the more wealthy and better fed individuals, were capable of imparting fevers of different types.

"Among the several thousands that were suffering from starvation(*a*), it had often struck me how comparatively rare were the ordinary types of fever. This I found afterwards to be explained by the fact that each class was liable to a particular form; thus the poor had the famine fever; the persons better off had the typhoid and modified typhus; and the mechanic, or the person in a corresponding class of life, accustomed to a better fare than the ordinary labourer, was usually attacked with a typhoid or typhus fever, but not presenting the usual well-marked gradations or stages observable in similar fevers in ordinary times. The convalescence was slow, and very frequently attended with a fatal termination, for want of the necessary nourishment. Again, in the upper and more independent class, the type assumed a new form altogether; here were found the gastro-bilious or jaundice fever, and well-marked typhus; or the former was characterized by commencing with the symptoms of ordinary simple continued fever of rather an aggravated kind, with some threatening of head and abdominal complications, when suddenly a jaundiced tinge of the skin occurred on the fourth or fifth day. The head symptoms increased; black sordes collected about the cheeks and gums; dark, fetid discharges came off from the bowels; and very frequently sudden effusion occurred within the head, causing instantaneous death: or else the case resolved itself, on the fifteenth or twenty-first day, by a critical perspiration. Typhus fever pursued its ordinary course in such cases, and was attended with its usual phenomena and complications.

"Dysentery had been by far more prevalent than fever in this locality. This was owing to the use of the various substitutes for the lost potato. In many cases the people resorted to sea-weeds of different kinds, and to a variety of molluscous animals found along the sea-shore, but more particularly to the

(*a*) "The calculation of our Relief Committee is, that we have at this moment 15,000 persons in a state of destitution."—*Note to Dr. Traill's published letter of 2nd January*, 1847.

use of Indian meal, which they were unaccustomed to, and did not understand how to cook properly; and in many instances they were obliged to eat it raw, for want of firing.

"The dysentery was very chronic, and attended with a great amount of suffering. The patients complained of excessive abdominal pain, painful tenesmus, and great thirst. The dejecta mostly consisted of pure blood and mucus. It was easily known, if any of the inmates in the cabins of the poor were suffering from this disease, as the ground in such places was usually found marked with clots of blood. The cases were most protracted, hopeless ulceration of the intestines always ensuing in spite of every effort to check the disease, except in those cases which were actively met in the outset of the attack by mercurial treatment, the only plan that my subsequent experience has proved to be of any avail. I myself suffered severely from an attack of this disease, and owe my recovery to this plan of treatment alone."

In this province no less than forty-eight medical men died in the year 1847, the great majority from fever(*a*).

ULSTER.

First Class of Queries.—The History and Antecedents of the Epidemic.

Antrim.—The fever was here unusually prevalent. In the towns of Belfast(*b*) and Antrim(*c*) dysentery and diarrhœa prevailed previously to the breaking out of the epidemic. Elsewhere in the county, for example, at Ballycastle(*d*) and at Ballymena(*e*), the general health of the population was previously

(*a*) See Drs. Cusack and Stokes' Report on the Mortality of Irish Medical Practitioners. Dr. Callanan of Cork,—whose report not having reached us until we were going to press, we are obliged to hold over till our next Number,—states that, in the year 1847, no less than 3329 deaths occurred in the Cork work-house.

(*b*) Dr. Malcolm.
(*c*) Dr. Brown.
(*d*) Dr. O'Connor.
(*e*) Dr. Black.

very good. In the two latter districts, the date of the commencement of the fever was January, 1847. In Belfast it began in May, 1847, and in Antrim in July of the same year. In the large towns most of those who were attacked had been suffering from the effects of starvation, and many were labouring under diarrhœa. "Individuals who were recovering from dysentery were peculiarly susceptible"(*a*). In the country districts those affected were generally in good health; the poorer classes, and sometimes the middle, were chiefly attacked. Change of diet among the people influenced disease considerably, especially in propagating dysentery; purpura or scurvy is stated to have frequently preceded the fever, in the reports received from Belfast and Ballymena, but not elsewhere in this county. No disease is stated to have been prevalent amongst cattle at the same time as, or antecedent to, the epidemic of fever, except in Ballymena. The greater number of those attacked were young persons, the average age being five-and-twenty. Females, in general, appear to have been more liable to the disease than males. In Belfast the proportion of cases to the population of the district was 16,000 in 110,000, or about one in seven(*b*): the proportion elsewhere was large.

Derry.—Dysentery prevailed in this county previously to the outbreak of the epidemic, which commenced in the spring months of 1847. It chiefly attacked those in a debilitated state from the effects of privation or bad health. The lower classes were chiefly attacked. No starvation existed in the city of Londonderry(*c*), and in the county only few instances of fever from its effects are recorded. Dr. Rogan did not meet in his practice with a single case of fever preceded by scurvy or purpura. Pleuro-pneumonia prevailed amongst cattle, both previously to and during the epidemic, in the northern and north-western parts of the county; but Dr. Vesey of Magherafelt states that it was not prevalent in his district.

(*a*) Dr. Black. (*b*) Dr. Malcolm. (*c*) Dr. Rogan.

The persons attacked by fever were usually between the ages of 15 and 30. There was no difference in the liability to the disease between males and females. As regards the query, "what was the proportion of cases to the population of districts," Dr. Rogan states "that no data exist to enable him to reply to it."

Donegal.—Dysentery, diarrhœa, and starvation prevailed in this county previously to the outbreak of the epidemic, which did not commence here until the month of November, 1847(*a*). The middle and lower classes were chiefly attacked, those affected being already lowered by general debility or in bad health. The disease often set in after recovery from starvation, but Dr. Little of Letterkenny states that he did not look upon it as an effect. The disease was sometimes preceded by scurvy. Young persons between 12 and 30 were chiefly attacked. Males appeared to be a little more liable to the disease than females, "in the proportion of 8 to 7"(*b*).

Tyrone.—The previous health of the district was good. The epidemic commenced in December, 1846. Those attacked were in ordinary health, but reduced in strength from want of food(*c*). The poor were principally affected, but the disease does not appear to have set in after recovery from the effects of starvation. It was frequently preceded by scurvy or purpura. The ordinary epidemic, pleuro-pneumonia, was prevalent among cattle previously to and at the same time as the epidemic fever. Young persons from 10 to 30 were those chiefly attacked with fever. Of the entire number of admissions into the Omagh Fever Hospital, 530 were females and 284 males. Of these ninety-one were under eleven years of age; 545 were between 11 and 30; sixty-one between 30 and 40; and 110 over that

(*a*) Dr. Little, Letterkenny, and Dr. Brady, Gweedore. Dr. M'Cormack of this county was one of the first to record the existence of true scurvy in Ireland. See his communication in the Dublin Hospital Gazette for 15th April, 1846. See also Dr. Curran's paper already alluded to at page 69.

(*b*) Dr. Little. (*c*) Dr. Hamilton, Omagh.

age. The whole population of this district is 69,099; so that the proportion of cases to the population was 1 in 85.

Fermanagh.—For the history of the epidemic in this county see the report of Dr. Betty of Lowtherstown in the provincial summary given at page 125.

Cavan.—Dysentery, diarrhœa, and anasarca were very prevalent in this county prior to the breaking out of the epidemic in March, 1847. "A previous state of good health had not much influence in warding off the disease; for although it weighed heavily on the poor, ill-fed portion of the population, whose constitutions were in fact so debilitated as to be unable to resist fever or any other form of disease that might present itself, we have observed it in some of the healthiest portion of the people, persons who were not subjected to privation in any shape, and who at the time of the seizure were in good health"(*a*). The poor were chiefly attacked, but clergymen and medical men also suffered severely. "In this county seven medical men died of fever within twelve months, and three had the disease in a very dangerous form, but ultimately recovered"(*a*). The disease very frequently set in after recovery from starvation. Change of diet did not appear to exercise any perceptible influence on it. Cases of purpura or scurvy did not occur in immediate connexion with fever, but purpura was frequently observed in this district in 1846. Pleuro-pneumonia was very prevalent amongst cattle previously to and during the epidemic. Of 1236 cases of fever under the care of Drs. Halpin and Mease, from whom the following statement has been received, 243 were under ten years of age; 442 from 10 to 20; 190 from 20 to 30; 152 from 30 to 40; 112 from 40 to 50; sixty-three from 50 to 60; and thirty-four from 60 upwards. Of these cases, 578 were males and 658 were females.

"The population of the town of Cavan and one mile round it was, as nearly as we can ascertain, 4736. The number of

(*a*) Drs. Halpin and Mease, Cavan,

cases was 1236; but of this number 236, or nearly one-sixth, had the disease twice, and ninety had it three times. The number is thus reduced to 915, or nearly one-fifth of the population; but this is not strictly correct, as many cases were brought in from remote country districts"(*a*).

Monaghan.—See report of Dr. Young at the conclusion of this province.

Armagh.—In the town of Armagh bad dysentery prevailed previously to the outbreak of the epidemic in April, 1847(*b*); but the county was not particularly unhealthy. In Loughgall the fever commenced in the end of 1846(*c*). The lower classes were chiefly attacked; the majority of those affected having been previously in bad health. The disease did not often set in after the recovery from the effects of starvation; but Dr. Leeper states that the epidemic materially declined as the poor were better fed. The fever was frequently preceded by scurvy. No epidemic disease was prevalent amongst cattle at the same time, or before the accession of the fever. Individuals at the age of puberty were chiefly attacked; but all ages were liable. Females were more generally affected than males at the first outbreak of the disease.

Down.—The health of this district was indifferent before the epidemic broke out, with the exception of the district of Downpatrick(*d*). In Newry, dysentery existed as an epidemic during the autumn of 1846, being very fatal among the old and infirm, who, if not carried off, were so debilitated by its effects as to render them an easy prey to the fever which followed(*e*). The fever prevailed greatly in the neighbourhood of Hillsborough(*f*),—particularly among the poor weavers,—where it did not appear until about the middle of June, 1847, but elsewhere in the county it appeared early in spring of the same year. Many of those attacked had previously suffered from the ef-

(*a*) Drs. Halpin and Mease, Cavan.
(*b*) Dr. Cuming.
(*c*) Dr. Leeper.
(*d*) Dr. Brabazon.
(*e*) Dr. Starkey.
(*f*) Dr. Croker.

fects of want of food. The epidemic chiefly affected the poor, but the better classes did not escape. Change of diet among the people appeared to influence the disease. In Downpatrick purpura and scurvy frequently preceded the fever. No disease appears to have been noticed as prevalent among cattle at this time. Dr. Croker writes that the greater number of those attacked with fever were from twenty-five to forty years of age, most of whom were females; while Dr. Brabazon states that one-half of his patients were between the ages of 12 and 30, and in the proportion of three females to two males.

Second Class of Queries.—The Character and Symptoms of the Disease.

Antrim.—The evidence as to the contagious nature of the late epidemic of fever is very decided, as reported from this county. In the upper and middle classes, head symptoms predominated, while in the lower classes abdominal complications and general symptoms of debility were chiefly present. Those cases that immediately followed on starvation were specially marked by debility. "The fever also was of a lower type, with great tendency to diarrhœa. Well-marked typhoid symptoms usually followed the starvation state early, the average period being the sixth day"(*a*). "The late epidemic differs from former ones," writes Dr. Malcolm, of Belfast, in the well-marked character of the types, which he describes as follows: "1st. Relapsing synocha—crisis, fifth to eighth day; relapse fifteenth; crisis, third day, with great debility. 2nd. Simple continued; crisis, ninth to twelfth, or more; low typhoid symptoms. 3rd. Typhus; crisis, eleventh to seventeenth day, or more; spotted generally; dry, brown tongue." Dr. Black also describes the chief peculiarity to consist in the number of relapses. Purpuric spots are said not to have been common in Ballycastle(*b*) nor in Antrim(*c*), but both in Belfast and Ballymena they

(*a*) Dr. Black. (*b*) Dr. O'Connor. (*c*) Dr. Bruce.

were generally met with. In the two former districts, crisis is also said not to have been common, while in the two latter it is remarked to have been frequent. Diaphoresis and epistaxis are stated to have been most frequent critical phenomena. Dr. Bruce reports that in his practice diuresis or diarrhœa also occurred occasionally as such.

Londonderry.—In this county the proofs of the contagious nature of the disease were also most convincing. Dr. Rogan reports that in the upper classes the brain was chiefly affected, in the lower, the chest and abdomen; and that the fever differed from previous epidemics in the frequency of chest affections, and the tendency to relapse. There were no starvation cases met with. Dr. Vesey, of Magherafelt, reports that purpuric spots were very general in the cases he attended; while Dr. Rogan says that petechiæ were common, but purpura rare. Well-marked crisis seems to have been very rare; when present, the most usual form of it was profuse perspiration, and epistaxis occasionally.

Donegal.—The evidence as to the disease being contagious was most conclusive. In Gweedore none of the rich were attacked(*a*). In Letterkenny(*b*) purpura and petechiæ were much more common in those previously suffering from want; also typhus symptoms appeared much sooner. In all starvation cases Dr. Brady states that the stomach and bowels were more affected than in others. Typhoid symptoms followed early the starvation state. In Dr. Little's experience the difference between the late fever and former epidemics "consisted in the early appearance of typhoid symptoms, the frequency of purpuric spots, and of relapses,—in some cases three and four times." Crisis was not common. The only critical phenomenon noticed was profuse perspiration.

Tyrone.—Numerous proofs as to the disease being contagious are reported from this county. Head symptoms were

(*a*) Dr. Brady. (*b*) Dr. Little.

more frequent in the better classes; dysentery and diarrhœa among the lower. In cases following starvation, typhoid symptoms, with petechiæ and purpura, were more frequent than in others not so circumstanced : these symptoms followed immediately. Dr. Hamilton, of Omagh, reports as his experience, that the chief characteristics of the late epidemic were "the frequent complications of dysentery and purpura, the tendency to relapse, and in being followed by pains resembling acute rheumatism." Purpuric spots were common. Crisis was very general, consisting in "perspiration or return of diarrhœa after being checked. I have often observed," states the authority already quoted, "a crisis after the application of the blister, without being able to account for it otherwise."

Cavan. — Drs. Halpin and Mease write : "We have no doubt of its contagious character. When the individual that was first attacked in a family was not immediately removed, the rule was, that every other member of that family became affected. We are in possession of a great number of cases in which the patients were perfectly sensible of having contracted the disease at the moment it had smitten them. For instance:

"No. 1. A medical attendant of a dispensary visited a patient in fever at her own house. On leaving this patient, he told his servant that, on coming down the stairs from the sick room, he was assailed by some extraordinary but abominable stench. He said he felt the fever seize him at the moment, and that he should never recover. He died on the ninth day after, of low typhus fever.

"No. 2. His successor visited a temporary fever hospital, of which he was in charge. On returning home, he told his wife that he had fever; he mentioned the patient's name from whom he had taken it. This patient had maculated typhus. He said he was aware of the moment of the imbibition of the poison. He felt that his system had received a shock from which he

never would recover. The patient alluded to died within two days of this visit. Dr. —— died on the twelfth day, of fever of a similar character.

"Several Roman Catholic clergymen in this locality suffered from fever; they were all able to detail the circumstances attendant on the contraction of the disease. All were sensible of the moment they became affected. They all died. A clergyman of the Established Church took fever from a patient in a filthy hovel in this town. He was aware at the moment that he was seized with it; it ran a course of fourteen days. He recovered.

"One of your reporters took fever in the hospital a very short time before it was closed. He felt the shock while standing beside the bed of a stout mason, who laboured under maculated typhus. This patient died within three days after. Your reporter's life was despaired of for many days. His fever terminated on the twenty-first day without perceptible crisis. The matron of our hospital had fever three times. One porter died of fever; another had the disease very severely. All our nurses, eleven in number, suffered; some twice, some three times. There was but one individual connected with this hospital, your other reporter, who did not suffer severely from fever while it was in operation."

In the higher classes the head was chiefly affected; in the lower, the stomach, bowels, and lungs. Cases arising after starvation were generally mild, requiring little treatment, and progressing favourably under the use of light nourishing diet, and a moderate quantity of stimulants. "Well-marked typhoid symptoms followed the starvation state in from two to three weeks." The chief peculiarity of the late epidemic in this county consisted in the short course of the primary fever and the frequency of relapse. Purpuric spots were observed in very few cases. "In primary cases where the patient was under twenty years of age, a crisis was usually observable on the fifth

or seventh day, and was most frequently ushered in by general perspiration"(*a*).

Armagh.—Evidence of contagion generally unequivocal; but Dr. Leeper, of Loughgall, states that, in his opinion, as regards the non-typhoid form of the disease, it was doubtful. In his practice, pure typhus without dysentery occurred in the better classes, and short fever with diarrhœa and dysentery among the poor. In Armagh head symptoms preponderated amongst the higher classes(*b*). Diarrhœa and dysentery were most frequent in cases following starvation. The particulars in which this fever differed from former epidemics were the shortness of the attack and the frequency of relapse. Purpuric spots were not common. Crisis was frequent amongst the lower classes; but not well-marked among the higher. Perspiration and sometimes diuresis or diarrhœa were the most frequent critical phenomena.

Down.—The evidence as to the contagious nature of the late fever in this county is positive. "I have no doubt," writes Dr. Hunter, of Bryansfort, Castlewellan, "that the disease was often propagated by contagion, especially among the poor, whose habits in every way tend to such a result. It was remarked, however, that among the upper classes, where attention was paid to cleanliness and ventilation, the disease, though appearing in a very malignant form in one individual, seldom attacked the attendants or other members of the family. Indeed its spread under such circumstances was very rare, but sporadic cases of fever often, in my experience, caused the disease in other persons by contagion." Dysenteric complications were most frequent amongst the lower orders; and dysentery and prostration marked those cases following starvation; well-marked typhoid symptoms following the starvation state usually on the fifth or sixth day. Purpuric spots were common in the months of May and June, 1847. "Crisis was not

(*a*) Drs. Mease and Halpin. (*b*) Dr. Cuming.

common; when present it was marked by sleep and perspiration"(*a*).

Third Class of Queries.—Complications.

Antrim.—Dysentery most frequently followed the fever, occurring in convalescence. In Ballymena(*b*) it very often preceded it also, but in Ballycastle(*c*) it was not present in a well-marked form. In Belfast(*d*) cerebral complications were the most frequent; in Ballymena, abdominal; and in Antrim(*e*) there were seldom any complications except among the upper classes, and in them the cerebral predominated. "In the epidemic at Ballycastle, the digestive organs and lungs were unusually free from congestion. The fever appeared to exert all its violence upon the nervous system, and those patients got on most favourably whose heads were blistered very early in the disease. The congestion of the conjunctiva was more marked than I had ever seen it in any epidemic"(*c*). Relapses were frequent, except in Dr. O'Connor's district; Dr. Malcolm reports that they occurred "almost regularly" in his first type (see p. 109). In relapsed cases the primary stage was short, "terminating in five days"(*b*), and "the relapses were generally more severe"(*e*). Petechiæ never appeared in relapse when they did not occur in the primary fever. The heart's action was feeble; "excessive debility of the organ in grave cases"(*d*); "sounds modified as in ordinary typhus"(*c*). In Dr. Malcolm's practice epistaxis was frequent, and generally occurred early; in Dr. Black's at the termination, or about the eighth day. No enlargement of the spleen was observed in any case, either during or subsequent to the fever. Bed sores were very rare in Antrim. In Ballymena sloughing over the sacrum and trochanters was frequent in old cases, and in Belfast sloughing and mortification over the sacrum, from pressure, sometimes occurred.

(*a*) Dr. Brabazon, Downpatrick. (*b*) Dr. Black.
(*c*) Dr. O'Connor. (*d*) Dr. Malcolm. (*e*) Dr. Bruce.

Derry.—Dysentery was prevalent at all times, and frequently preceded the fever. In Magherafelt(*a*) the abdomen and head were chiefly engaged. From Derry Dr. Rogan writes, that the complications occurred there in the following order, as regarded their comparative frequency:—1st, bronchitis; 2nd, dysentery; 3rd, pneumonia; 4th, jaundice; and that relapses occurred in about a third of his cases. Elsewhere in the county they were also very frequent. In Londonderry the duration of the primary fever, in relapsed cases, was much longer than it appears to have been in other parts of Ireland, "lasting from nine to twelve days"(*b*), while the relapses were seldom longer than four. Petechiæ very rarely appeared in relapse when they did not occur in the primary fever. No morbid phenomena of the heart or spleen are noticed as having been present. Epistaxis was occasionally present at the crisis. Bed-sores were frequently met with by Dr. Vesey, terminating in gangrene and sloughing, often to a considerable extent. Dr. Rogan did not meet with them frequently. They were chiefly in the old.

Donegal.—Dysentery nearly always accompanied the fever. Gastric complications were most frequent. Relapses were nearly always present, "sometimes occurring three or four times in the same case"(*c*). No peculiarity was noticed as having been present in those primary cases which were followed by relapse. Petechiæ were observed by Dr. Little, but not by Dr. Brady, in relapse, when they did not occur in the primary fever. No morbid phenomena of the heart or spleen were noticed by the latter physician; but Dr. Little observed that, although the heart's sounds were normal, its action was always weak. Epistaxis was not frequent. "I only had it," observes the same physician, "in one case, which I considered convalescent, when hemorrhage suddenly occurred from nose, mouth, bladder, and

(*a*) Dr. Vesey. (*b*) Dr. Rogan. (*c*) Dr. Little.

intestines, with large purpuric spots over the entire skin and mucous membranes, as far as visible. It terminated fatally."

Tyrone.—Dysentery was frequent in all stages, "preceding, accompanying, and following the fever"(*a*). Gastro-intestinal complications were the most frequent. Relapses were of very common occurrence, "the relapsed stage being of shorter duration than the primary fever"(*b*). Petechiæ did not appear in relapses when not present before. No morbid phenomena of the heart or spleen are noticed as having been present. Epistaxis was frequent about the middle period of the disease. Bedsores were not often met with. When they did occur, they were in the form of very deep sloughs.

Cavan.—Dysentery was not of frequent occurrence. It appeared in a very few instances, and of a very fatal form, as a sequela to the disease. The most usual complications were a low degree of inflammatory action in the stomach, congestion of the lungs, and congestion of the liver. Relapses were very frequent. In the relapsed cases, the prostration was extreme, and the subsequent recovery protracted. Petechiæ did not appear in relapse when they did not occur in the primary fever. "The most striking morbid phenomenon of the heart was extreme slowness of its action, in nine cases the pulse remaining so low as from forty to fifty beats in a minute during the entire progress of the disease, notwithstanding the copious administration of the strongest stimulants; yet all these cases terminated favourably." Epistaxis occurred occasionally towards the termination of the disease, and, when it did, was generally accompanied by hemorrhage from other parts of the body. Enlargement of the spleen was not observed, and bedsores were not frequent(*c*).

Armagh.—The dysentery both preceded and followed the

(*a*) Dr. Hamilton, Omagh. (*b*) Dr. Hamilton.
(*c*) Drs. Halpin and Mease.

fever. Diarrhœa was the most general complication, occurring in about fifty of the cases at Loughgall(*a*). In almost every case of famine fever relapse occurred, but "not among the higher classes"(*b*). Dr. Leeper frequently observed that the second sound of the heart was absent in many cases, and that its return was a favourable change. Epistaxis occurred occasionally in the middle stage. "It was sometimes followed by relief, but at other times, even at an early stage of the disease, it was considered an unfavourable symptom, indicating a broken-down condition of the blood"(*b*). Bed-sores were very rare.

Down.—Dysentery was very frequently present, before, during, and after the fever. "Gastro-enteric complications were the most frequent; next, congestion of the head, and then of the chest"(*c*). Relapses were very frequent, and more protracted than the primary cases. Petechiæ were sometimes noticed in the relapse, though they had not been previously present. Dr. Brabazon mentions that he noticed frequently a feeble impulse of the heart, with voluminous pulse at the wrist. Epistaxis was frequent early in the disease. Bed-sores were very infrequent.

Fourth Class of Queries,—including the Mortality and morbid Appearances.

Antrim.—The mortality was greater in the upper classes. In Ballycastle(*d*) it was 1 in 5 in the middle classes. In the Belfast General Hospital(*e*) it was nearly 1 in 8,—but this included dysentery cases, which were unfortunately mixed up in the registry with the fever cases. In Belfast, where alone *post mortem* examinations appear to have been made, nothing remarkable is noticed as having been present. No data exist in that city from which a comparison could be drawn as to the difference in the mortality among those attended in their own

(*a*) Dr. Leeper. (*b*) Dr. Cuming. (*c*) Dr. Brabazon, Downpatrick.
(*d*) Dr. O'Connor. (*e*) Dr. Malcolm.

houses or in hospitals. In Ballycastle it was greater among the former, and in Ballymena(*a*) among the latter. The towns and densely populous districts always exhibited a greater amount of mortality than the less thickly populated places. Dr. Black remarks also that it was greater in low marshy districts.

Derry.—In this county the fever was more fatal among the upper classes. Dr. Rogan states that in the cases examined by him after death, no constant or peculiar lesion was observed. The mortality was much greater in those attended at their own houses than in hospital. Dr. Vesey reports that there was no difference in the amount of mortality in the town and rural districts. Dr. Rogan states that it was greater in the latter, "owing," he adds, "probably, to the facility for the early removal of patients to fever hospitals in town."

Donegal.—In Letterkenny(*b*) the mortality was $8\frac{1}{2}$ per cent. in all cases, but greater in proportion in those previously worn down by disease. It was only 1 per cent. in Gweedore. No *post mortem* examinations were made. There is reported to have been no difference in the per centage of the mortality in town and country districts.

Tyrone.—In the Omagh Fever Hospital the mortality was 92 in 814 cases, about 1 in 9. No opportunity was afforded of making *post mortem* examinations. The mortality was equally as great in densely as in thinly populated districts.

Cavan.—"In the upper classes the mortality amounted to at least 66 per cent. In the Cavan temporary Fever Hospital, which was opened on the 9th of April, 1847, and closed on the 10th February, 1848, out of 1236 cases forty-eight died, being a mortality of $4\frac{1}{4}$ per cent. No *post mortem* examinations were made"(*c*). The mortality was greater in those attended in their own houses than in hospital; and it seemed to depend rather on the crowding together in houses than on any peculiarity in the district.

(*a*) Dr. Black. (*b*) Dr. Little. (*c*) Drs. Mease and Halpin.

Armagh.—The mortality was proportionally greater among the higher classes. It was also much greater in those attended at their own houses. No *post mortem* examinations were made.

Down. — Dr. Brabazon reports that the mortality was greater among the destitute. Nearly all the cases in his district were sent to hospital. He states that the town and densely populated districts in his neighbourhood, beyond doubt, exhibited a greater amount of mortality than the thinly populated rural districts. No *post mortem* examinations were made.

Fifth, sixth, and seventh Classes of Queries,—relating to the Duration of the Disease, Modifications, and Sequelæ.

Antrim.—The usual duration of the disease, according to Dr. Malcolm, varied in the different types described by him. The first lasted for eight days, and the relapse for three or four days; the second for ten or twelve days; and the third for fourteen days, and sometimes longer. In Dr. Black's two types, the duration of the first was about nine days previous to the relapse; the second about fourteen days, which appeared to be the average duration of the fever in other parts of the county. The stage of convalescence was in general very tedious, lasting frequently for a month, or even longer; but in Dr. Malcolm's first type it was very short, seldom lasting longer than a few days. The primary stage of those cases followed by relapse was usually short. In the Belfast Hospital the original cases were distinguished by high fever, very white tongue, and flushed face. In Ballymena, the modifications of the symptoms noticed in individuals of the same family were, in the aged, petechial fever, in the young, relapses. Dr. Malcolm has also noticed that the younger were more liable to relapse; and Dr. O'Connor, of Ballymena, mentions that in his practice the fever in grown persons generally preserved its type in a very remarkable degree. Erysipelas, dysentery, abscesses, and gangrene,

were the usual sequelæ met with in the Belfast Fever Hospital. In Ballycastle, sequelæ of any kind were not frequent.

Derry.—The duration of the fever varied from about twelve to twenty days, "and from nine to twelve days in relapsing fever"(*a*). Convalescence was very slow in general, often lasting for several weeks. "These cases," writes Dr. Rogan, "which were followed by relapse, were of a peculiar character, differing essentially from true typhus." The symptoms occurring in different members of the same family were pretty uniform. Dr. Vesey, of Magherafelt, describes abscess, diarrhœa, and dropsy, as the usual sequelæ of the fever.

Donegal.—In this county the duration of the fever was unusually long, and the convalescence very protracted. Dr. Little reports that the disease lasted from "five to seven weeks, including convalescence," in his district; but Dr. Brady states that the duration of the fever was "from three to six weeks, and the convalescence from four to eight weeks longer, from want of food," in that part of the county where he resides. Diarrhœa, anasarca, and erysipelas of the head were the usual sequelæ that occurred in Dr. Little's practice. He also mentions that small-pox followed many of his cases.

Tyrone.—The fever lasted for about fourteen days, and the convalescence from two to three weeks longer. The usual sequelæ were arthritic pains very frequently, and in some cases deep-seated abscesses in or about the parotids, and a few instances of sloughing of the mouth and cheeks(*b*).

Cavan.—The average duration of the disease was about twenty-four days; the convalescence about ten days longer. In those primary cases which were followed by relapse, the disease usually ran its course very mildly in five or seven days. "There was a remarkable similarity of symptoms in members of the same family, and we remarked that two or more of the

(*a*) Dr. Rogan. (*b*) Dr. Hamilton, Omagh.

same family were generally convalescent at the same time; if one of them relapsed, every one of the others followed the same course, and it was not infrequent to find them all relapsed on the same day"(*a*). Anasarca, swelling, and dysentery were the usual sequelæ.

Armagh.—The duration of the disease was from about a fortnight to three weeks, and the convalescence about the same length. In Loughgall(*b*), the primary cases, which were followed by relapse, lasted for eight days. There was then partial convalescence, and then relapse. Dysentery was the most usual sequela.

Down.—The fever usually lasted for about a fortnight, and was followed by a convalescence of from one to three weeks. Anasarca and phthisis were the most usual sequelæ.

Eighth Class of Queries,—referring to the Treatment.

Antrim.—The reports from this county are unanimous in stating that a stimulant plan of treatment was usually found most serviceable, and in many of them it is mentioned that the use of stimulants was required early in the disease. Wine was largely employed, and its administration seems to have been looked upon as essential. General bleeding was not resorted to, but local was used in congestions; "when employed early in head symptoms, it was attended with good effect"(*c*). Mercury was used by Dr. Malcolm in all local inflammations of parenchymatous organs. It does not appear to have been given in any case to produce its specific action. Opium was employed where dysentery or diarrhœa prevailed, and also with a view of producing sleep. "Opium, with tartar emetic, as recommended by Dr. Graves, was employed in many cases, when sleep could not be obtained by other means; and this treatment succeeded in almost every case where it was had recourse to"(*d*).

Derry.—A stimulant plan of treatment was found most suc-

(*a*) Drs. Mease and Halpin.
(*b*) Dr. Leeper.
(*c*) Dr. Black, Ballymena.
(*d*) Dr. O'Connor, Ballycastle.

cessful. Wine and other stimulants were used largely. The following is an outline of the method employed by Dr. Rogan of Derry: "In uncomplicated fever, the general treatment consisted in attention to the bowels, cleansing the skin, shaving the scalp, and the use of mild diaphoretics; where cerebral affection was well marked, leeches to the temples, cold to the scalp, blisters to the neck, and tartar emetic, combined with laxatives. When bronchitis was present in young subjects, and in the early stage, general bleeding was occasionally used. In both old and young, tartar emetic, combined with small doses of tincture of opium, and, in the advanced stages, cordials, stimulants, and blisters. When pneumonia occurred, general bleeding, calomel, with tartar emetic and blisters to the chest; in the complication with dysentery, opium, with the milder preparations of mercury." The same physician also mentions that the employment of general bleeding seemed to protract convalescence. Elsewhere in the county, bleeding seems to have been very rarely employed in any form. Mercury was not used to produce its specific effects. Opium was chiefly given where dysentery appeared.

Donegal.—A stimulant plan of treatment was here also found most serviceable. "Wine and other stimulants in large quantities, with opiate enemata, and acetate of lead and opium internally, to check the diarrhœa which occurred in almost every case at some stage, constituted the treatment I employed"(*a*). General bleeding was never had recourse to, and local only occasionally, to meet head symptoms. Mercury was used merely as an alterative, but Dr. Brady employed it in the first stage, when dysentery was present. When this complication occurred in the middle and last stages he used opium, which was also employed for the same purpose by Dr. Little, and with benefit.

Tyrone.—The stimulating treatment was found most ser-

(*a*) Dr. Little, Letterkenny.

viceable in this county, and was very early required. Dr. Hamilton adds, that blisters to the abdomen were very useful, and also that he used whiskey as a stimulus, which he found to agree much better with his patients than wine. Local bleeding was alone employed, but even in the form of leeching over the epigastrium it was not well borne. Mercury was but seldom administered. "Opium was much used, and much earlier than formerly, with the best effects; combined with tartar emetic in cases such as recommended by Dr. Graves, many lives have been saved that would otherwise have been hopeless"(*a*).

Cavan.—Stimulants were in this county also found most useful. Combined with them Drs. Mease and Halpin employed counter-irritation over the stomach, thorax, or entire abdomen, either by blisters, fomentations of hot turpentine, or mustard. Hydrargyrum cum cretâ, in small doses, frequently repeated, but not pushed to salivation, with or without Dover's Powder, was found very serviceable. "Blood-letting," say they, "was resorted to only in six cases; in one, where the fever was complicated with pneumonia, small bleedings were practised with relief to the distressing symptoms. In three cases no appreciable benefit followed, and in the remaining two we were of opinion that it was injurious. In the first case the bleeding was from the arm; leechings and cuppings were employed in the others. Calomel, combined with extract of hyoscyamus, opium, or morphia, was frequently had recourse to in irritability of the stomach, with great benefit. In a few cases this combination produced ptyalism very rapidly, which we did not desire, as we had no faith in mercurialization in checking the disease, or rendering it more tractable. Where salivation was thus accidentally produced the recovery was protracted greatly, and the constitution seemed to be so much enfeebled that, when a relapse occurred after, the patient not unfrequently sank. Opium was generally used as a sedative at

(*a*) Dr. Hamilton.

night, where head symptoms did not preclude it. These cases were few. The preparations most relied on were Dover's Powder, acetum opii, and acetate and muriate of morphia; these latter appeared specially applicable to gastric irritation."

Armagh.—Dr. Leeper's plan of treatment was expectant. Dr. Cuming, of Armagh, has forwarded the following report of the method adopted by him:

"When the disease was seen at its very beginning, upon the first or second day, it was sometimes averted by the antimonial solution given so as to act freely on the stomach, bowels, and skin. In other instances, particularly where there were any bronchial complications, a grain of calomel with one of ipecacuanha, every three or four hours, was productive of benefit, more particularly if the mercurial effect could be speedily established in the system. If the mercury did not produce its specific effect in the course of a few days, I discontinued it altogether, as I consider that when it does not soon act as a *remedy*, by a further perseverance in its use it may come to act as a *poison*.

"Cold applications to the shaved scalp, and a *moderate* detraction of blood, by means of leeches, from the temples, when there was evidence of cerebral congestion, were often of service at an early stage of the disease. As the fever advanced, however, the treatment was for the most part of an expectant nature, and it was very generally found necessary to support the strength by wine, arrow-root, chicken broth, and beef tea. At an advanced period of the disease benefit seemed frequently to result from the application of a blister to the nape of the neck, crown of the head, or between the shoulders, according as there was evidence of congestion in the brain or lungs."

Wine was freely employed and preferred to other stimulants. General bleeding was never resorted to, and local very rarely, not at all by Dr. Leeper, who also very seldom gave mercury, and, when he did use it, did not think it productive of good effects. Opium was employed in the advanced stage of

the fever, and nearly always with good results, especially when there had been much sleeplessness, delirium, and nervous irritability. "A full opiate," says Dr. Cuming, "combined with an antimonial, I often found to be attended with the most beneficial effect."

Down.—A generally stimulant plan of treatment was found most serviceable. "Wine and other stimulants," writes Dr. Brabazon, of Downpatrick, "were used extensively and necessarily in the greater number from the moment of admission, in many before removal from the conveyance into the hospital." General bleeding was not resorted to, and local only very rarely. Mercury was given frequently in hepatic congestion, and sometimes in dysentery. Opium was occasionally administered in combination with tartar emetic, head symptoms prevailing. It was very often employed in dysentery.

Summary of Reports not comprised in the foregoing Details.

The character of the epidemic in a portion of the county of Fermanagh, may be learned from the following observations of Dr. Betty, of Lowtherstown :

"In the early part of the summer of 1847 I had extensive opportunities of seeing this fever in all its stages, having charge of the Union Fever Hospital of forty beds, and a very large dispensary district, which seldom at any season wants fever of typhoid character. That the then epidemic was contagious I do not think any person who witnessed it could have a doubt, particularly amongst the poor. It was highly so in their wretched abodes, where there is such want of cleanliness, and, I may say, careless indifference about fever; as, when the disease is once pronounced fever by the medical man, the poor seem to think it will run its course, and in most instances will not attend to either medicine or medical advice, although all or a greater part of the family may be attacked. With the better classes this disease has seldom gone farther than one case in a family

(in this county), which is, no doubt, owing to cleanliness, caution, and attention to free ventilation.

"I have remarked at the workhouse that persons coming in while in a state of starvation were often taken with fever, and in many instances had tedious recoveries. I think that in the better classes the head was much more affected than in the poor. The medical profession suffered more from this fever in this county than any of the better classes. I had a very severe attack myself; crisis on the thirteenth day without any visible phenomenon; my tongue was moist all the time (I understand); my head was greatly affected, and I had petechiæ on the fifth day. I was well aware of the time I caught the disease,—after visiting a man who had lately come from Scotland: I felt ill on my way home. The worst class of fever we had was that which was imported from Scotland, and the common people were well aware of it, and called it the Scotch or black fever, from the great darkness of the countenance during the attack. In a few instances dysentery followed fever in my hospital, and was generally fatal."

(*To be Continued.*)

[We are compelled by the necessary mechanical arrangements of our Journal to close this portion of the Fever Report thus abruptly. It will be continued in the next Number. In the mean time we would feel much obliged by our country friends, particularly in the West, who have not yet answered our Circular, doing so with as little delay as their convenience will permit; and we would still gladly receive additional communications from the North and South.—Ed.]

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Medical Jurisprudence. By Alfred S. Taylor, F. R. S., Professor of Medical Jurisprudence and Chemistry in Guy's Hospital. Third Edition. London, Churchill. 1849. 1 vol. Fcap. 8vo. pp. 849.

In an early Number of our New Series the second edition of Professor Taylor's Jurisprudence was reviewed at length; and more recently we had the gratification of bringing before our readers his admirable Treatise on Poisons, and expressing our opinion of its merits. The publication of either of these works would be sufficient to place him in the foremost rank of medical jurists, and the authorship of both shows that his industry is equal to his talent. The present edition has undergone considerable alteration and improvement, notwithstanding the short time that has elapsed since the second was published. Much of it has been rewritten, eight new chapters have been added, the table of contents and index greatly enlarged, and the work increased in size by the addition of more than 100 pages: in short, every new fact of importance in the science, together with a careful selection of cases in their medico-legal relations, from those recorded in the United Kingdom, on the continent of Europe, and in America, has been added, and the volume thus rendered a faithful and trustworthy exponent of the present advanced state of this important department which so closely unites the medical and legal professions.

The subject of poisons, too, although treated at considerable length in the volume the author has specially devoted to this branch of medical jurisprudence, is not neglected in the work now before us. In it is contained an admirable summary of his more extended observations, quite sufficient for the general purposes of the physician and the barrister. Mr. Taylor's two volumes, printed in the clear and beautiful type of Mr.

Churchill's manuals, may be truly said to constitute an encyclopædia of information on forensic medicine, and cannot fail to promote the cultivation of this most important division of medical study.

The additions under the various heads to the present edition of his Treatise on Jurisprudence are thus stated in the preface:

"In the section on wounds, additions have been made on the following subjects:—the medico-legal examination of wounds; physical, chemical, and microscopical evidence in reference to blood stains; on imputed or self-inflicted wounds; medico-legal facts connected with the fatal results of surgical operations; medical responsibility and malapraxis; death from the entrance of air into the veins; ruptures of the kidney and urinary bladder; spontaneous fractures; wounds from fire-arms not loaded with ball; varieties of burns; injuries from melted metals; and on injuries (simulating wounds) caused by fire.

"In the section on infanticide:—on the death of the child before respiration; on the alteration in the chemical composition of the lungs by respiration; on marks of violence, and on the detection of food in the stomach as signs of live birth; additional observations on the hydrostatic test; on the newly discovered fallacies of the *docimasia circulationis;* the congenital closure of the ductus arteriosus and foramen ovale; and on the occurrence of severe accidental injuries to children during birth.

"The chapters on pregnancy and delivery have been almost entirely rewritten. Under these heads will be found additional cases and observations on the plea of pregnancy, and on the signs of delivery in the dead in cases of criminal abortion. Under this branch of the subject, the new views of physiologists regarding the evidence derivable from the presence of corpora lutea are also fully considered.

"Many additional facts have been recorded in reference to the medico-legal bearings of the Cæsarean operation, Tenancy by Courtesy and Age and Minority. The chapter on poisonous gases and vapours has been in great part rewritten. In the section on insanity there will be found among the additions the decision of the judges on medical certificates, with new cases and observations."

This extract shows some among the many improvements in the present edition of Dr. Taylor's treatise; it will also prove, we are certain, the necessity which each and all of our readers must feel of becoming, for their own sakes, acquainted with its contents. We need scarcely again repeat our opinion of the value of this volume to the two professions; of this the statement in the preface, that since the first appearance of the work in 1844 no less than 4000 copies have been sold, is much stronger testimony than any we could offer.

1. *The Ethnological Journal; a Monthly Magazine of Ethnology, Phrenology, and Archæology, considered as Elements of the Science of Races, with the Application of this Science to Education, Legislation, and Social Progress.* Edited by Luke Burke, Esq. London, Ballaire. Parts I., II., III.

2. *Lectures on the Races of Men.* By Robert Knox, M. D., F. R. C. S. Published in the Medical Times.

3. *The Natural History of the Human Species: its Typical Forms, Primæval Distributions, Filiations, and Migrations. Illustrated with thirty-four coloured Plates, &c.* By Lieut.-Col. Charles Hamilton Smith, K. H. Edinburgh, Lizars. Small 8vo., 1848. pp. 464.

Science achieves its greatest successes in dealing with the less noble materials. We can predict the movements of the heavenly bodies, weigh the stars, and measure the forces which excavate the craters of the lunar volcanoes; but the law of gravity which gives these amazing results is only conversant with brute matter. If we rise to the phenomena of life, or ascend still higher into the region of mind, our exact formulas have to be abandoned; and we must be content with a less absolute nomenclature, and humbler conclusions. The facility, however, of systematizing in the lower material, naturally and almost inevitably invites us to deal in the same way with the loftier and more subtle phenomena which surround us as we rise to the contemplation of life,—vegetable, human, or spiritual. We see economists thus daily endeavouring to discover and apply laws as rigid as those of crystallization, or any other phenomenon of brute matter, to social affairs, involving not only the animal functions and instincts, but the sentiments of the heart, and the faculties and aspirations of the human intellect and soul. So again, in the division and classification of the families of men, we observe the tendency to systematize and generalize outrun all reason and experience. We have before us two remarkable instances of this disposition among philosophic minds, in the Ethnological Journal of Mr. Luke Burke, and the Natural History of the Human Species, by Colonel Hamilton Smith. The Journal, we think, was wanted; and so was a popular treatise in which we might have Prichard condensed, and possibly improved. But the aim of a Journal, taking Ethnology for its subject, should rather be to collect material for future classification, than to reduce the future communications of new phenomena to a pre-arranged system.

Mr. Burke, however, has prepared in his first Number a Procrustean bed, into which whatever may afterwards turn up will have to be thrust and fitted, at whatever inconvenience of stretching or amputation; and in this we think he is in no way to be commended. We pass by his division of the *genus* man, into harmonic and discrepant; but, taking up the harmonic half of the genus (that is to say, man proper, as contradistinguished, in this revival of Lord Monboddo's views, from the ape), we have the harmonic man divided again into two branches, denominated the *intellectual* and the *physical:* the former, which indicates the Caucasian type, affecting intellectual pursuits as the main business of life; and the latter, which seems to represent the Negro variety, having for their primary objects of existence physical exertion and animal enjoyment. Such a division at starting would entail a good deal of arbitrary arrangement, as further facts might come in; but what shall we say of the prospect of embarrassment which Mr. Burke has before him, when we follow up his anticipatory classification a little further?

"The intellectual races are divisible into nine species, each having two varieties, and five supplemental or intermediate species, each with two varieties: in all twenty-eight varieties or distinct races. The supplemental varieties, though essentially intellectual, nevertheless approximate, in some respects, to the distinctive character of the physical races.

"The physical races are divisible into four species, each of which has from four to twelve varieties; and there are besides supplemental varieties; the whole making thirty-five races of this branch.

"Thus the total number of the primitive races of the human family is sixty-three."

Well indeed might our author deem it prudent to add as he has done:—"It must be observed that the minutiæ of these classifications are in part made upon theoretical and systematic grounds; and therefore that they must be considered as in some degree conjectural, and subject to future modifications."

It would appear from this preliminary indication of Mr. Burke's views, that he regards his sixty-three diversities of the human family as referable to two principal varieties, represented by (we suppose) the Negro on the one hand, and the Caucasian on the other. The usual division recognised by naturalists who have not felt themselves, in this regard, bound by the statement of revelation, has been a tripartite one. Indeed, regarding mankind as sprung from the three sons of Noah, we see nothing inconsistent with the sacred narrative in assuming Ham to have been the parent of all the

Negro families. It is true this assumes something more than a *sporadic* diversity among the sons of Noah; and those who insist on specific differences, cannot be expected to reconcile their views to holy writ by such an accommodation. But Mr. Burke labours under no difficulty or scruple on this account; he enters, in his first Number, on a bold denial of the authority of Scripture in all matters of the kind. The point on which he assails the sacred volume is that of chronology,—a point, indeed, on which the enemies of infidelity have very often placed themselves and their friends in the wrong, by insisting on the testimony of the authorized (or indeed, on this point, of any) version, against the plain evidence of geological and other facts. It is very well known to all biblical scholars (although our preachers have not usually deemed it necessary to occupy the attention of mixed audiences with these considerations from the pulpit) that the books which constitute the Old Testament remained for shorter or longer periods committed to manuscripts of a very imperfect kind of writing, in which the consonantal letters only were expressed, the vowel sounds being left to be filled in, according to the context. It is equally well known that the three earliest versions of the books forming the Old Testament, viz., the Hebrew, the Samaritan, and the Septuagint, differ in their antediluvian chronology to a great and irreconcilable amount. These facts have generally inclined biblical scholars, who really know the nature of the evidences about which their science is conversant, to regard very tolerantly the large demands of geologists and other scientific inquirers, who insist on a still longer period than even (the most extended) that accorded by the Septuagint text, for the performance of those phenomena, which, they allege, must have been transacted on the surface of the earth, both before and since man's creation. It is true, some ill-read and narrow-minded men, ignorant of biblical science, but sufficiently distinguished among their more ignorant followers to be deemed worthy to act as annotators of new editions of the Bible, have insisted on the literal exactness of the chronology of the Authorized Version, in a way as irreconcilable with the ancient versions and with the facts of science, as it is inconsistent with ordinary prudence; but the number of these zealots is small, and scientific inquirers have the satisfaction to know that there is nothing insisted on by Christian divines of learning, hostile to, or irreconcilable with the longest periods required either for the successions of geological phenomena, or for the growth and diffusion of the families of mankind over the globe which they inhabit. On the contrary, the liberal

divine, yielding to the geologist the periods he requires, receives back from geology the most convincing confirmations of the order and series of creation having been truly detailed in Genesis; while the candid historical critic, if he have to depart from the chronological canon of either or any version, will willingly admit that the preservation, during a period of 1200 years, of a perfect agreement, in matters merely numerical, between the different copies of a manuscript committed to a mode of writing so imperfect as the unvocalized Hebrew, is more than human reason could expect, or ordinary candour would demand. Mr. Burke, however, seems to think it necessary *in limine* to get rid of all the biblical records of the human race anterior to the deluge; a preparation which we need not say argues badly for the reasonableness of his further speculations.

While we thus freely censure Mr. Burke for what strikes us as an unphilosophic effort at system in a subject so difficult to be systematized, and for a needless and ostentatious dissent from authorities not necessarily in antagonism to any scientific truth, we cannot but give him credit for having supplied the want of a vehicle for facts and speculations, in one of the most interesting inquiries that can occupy the mind of man. For while the flowers of the field, the shells of the sea, and even the seaweeds that strew the shore, have had their respective chronicles and repositories, man himself, the lord and governor of all, exhibiting greater diversities and more wonderful discrepancies, in his unity, than any other created object, has, until very lately, remained without any work specifically devoted to his own illustration. But the reason is obvious. It is easy to classify shells and flowers; a little less easy, but still very practicable, to classify birds and mammals: but to trace the origins and affiliations of families of men, with their diversities, not only of physical characteristics, but also of speech and thought, of habits of mind and feeling, of modes of life and social economy, this is extremely difficult—arduous—demanding the highest attainments, not only in physical science, but in history, philology, and morals. For he who would comprehend man (not as a mere zoologist, but in an ample, philosophic way), must be master, through man, of all that man has been created to be, to do, to suffer, and to preside over Speech alone (not to speak of thought, feeling, and faith), that lowest faculty which puts the difference between our race and the inferior animals, has exercised the intellects of the most profound scholars and philosophers, and has overtasked them. That the pursuit of ethnology, then, should have been neglected, is not

so surprising as that it should have been so boldly prosecuted, and, up to a certain point, prosecuted with so much success. For, whether arising from original specific differences, or from accidental diversities increased by local and social circumstances, so as to constitute a second nature, the human race has now been satisfactorily distinguished into several well-marked separate families, whose characteristics of person, speech, and even mind, exhibit remarkable and permanent differences. Whether any of these families be mentally or morally inferior to the others, is a question still unsolved; but the current of opinion, now running counter to the course it took some years ago in this eddy of science, tends every day more and more to affirm that such an inferiority exists, and is likely to continue until the inferior races be trodden out of existence by the march of the superior ones.

Although we are far from assigning to Mr. Burke an eminent place in the science,—thinking him, as in plain terms we admit we do, rash and insufficiently informed,—we willingly give him credit for having opened some views of social polity in connexion with this question worthy of the gravest consideration both by the philosophic historian and the statesman. The natural equality of mankind Mr. Burke denies; and on the contrary asserts not only an innate superiority over the other races for the Caucasian variety, but amongst the Caucasians themselves an innate superiority in one family over another, and in the aristocracies of individual families themselves over the masses. We make no doubt Mr. Burke has pushed his theory past the verge of philosophic induction; but when society suffers such grievous evils as now afflict the oldest and most famous seats of civilization, for the sake of "equality and fraternity," it is worth while to consider whether such principles exist in nature, or whether mankind may not in this, as in many former revolutions, have engaged in the overthrow of authority in order to exalt a delusion to the place of an axiom. It is true the doctrine is usually put forward with a religious sanction:

> "Politicians have taken it for granted that men are equal and alike by nature, because scientific men have generally said they were so; and scientific men have generally said they were so, because theologians have told them that to say the reverse would be to contradict the word of God. Thus a question of fact has been settled by authority, and a question of science by unscientific men."

Remembering how theology formerly sought to interfere with astronomy and navigation, and seeing with what difficulty, even in our own time, geology has raised itself against the denunciations of a section of the less learned clergy, we are willing

to see the theological influence confine itself as strictly as Mr. Burke himself could desire to the spiritual domain. But as in geology, so in ethnography. The Scriptures themselves will not be found inconsistent with any results of truth. If science should conclude that one branch of the human family is of an inferior nature, physical or moral, the simplicity of faith will neither be scandalized nor surprised. In the curse pronounced against Canaan we have abundant indications of an unequal dispensation to reconcile the written word with any differences that may be shown still to exist; and if such differences should turn out to be illusory, no candid reader of the Bible will repine at having to class that passage of Genesis with the other portions of the Old Testament writings which have lost their force and significancy in the milder dispensation of the New. The idea of original equality is not peculiarly a Christian one. Borrow, in his Bible in Spain, gives a remarkable and beautiful instance of the same sentiment in the breasts of Moslems:

"A large boat now drew nigh, the stern of which was filled with Moors. Some of them were exceedingly fine men, and two amongst them, youths, were strikingly handsome, and, so far from exhibiting the dark and swarthy countenance of Moors in general, their complexions were of a delicate red and white. The principal personage, and to whom all the rest paid much deference, was a tall, athletic man of about forty. The Spanish sailor informed me, in a whisper, that he was a Santuron or big saint, and was so far back on his way from Mecca; adding, that he was a merchant of immense wealth. It soon appeared that the other Moors had merely attended him on board through friendly politeness, as they all successively came to bid him adieu, with the exception of two blacks, who were his attendants. I observed that these blacks, when the Moors presented them their hands at departing, invariably made an effort to press them to their lips, which effort was as uniformly foiled; the Moors, in every instance, by a speedy and graceful movement, drawing back their hand locked in that of the black, which they pressed against their own heart; as much as to say, 'though a Negro and a slave, you are a Moslem, and, being so, you are our brother. Allah knows no distinctions.'"

But Mr. Burke pursues his idea of specific differences to a much greater length, than in the mere case of national disparities; and contends for the essential superiority of every aristocracy of pure blood over the mixed breeds of the common people. All the ancient republics, he contends, were aristocratic, the power of the nobles being great in proportion to the purity of their blood, and the prosperity of the state commensurate with that power. The rise of the common people to political

influence he conceives to have been ever attended with turbulence, civic impoverishment, and national weakness. At first sight it seems singular that a man of philosophic mind, writing in England, and with the example of America before his eyes, should advocate these opinions. But if we survey the field of history with a comprehensive view, we shall find evidences enough of the main proposition insisted on by Mr. Burke, that in all falling states, and "in all popular convulsions, the half-caste is ever the cause and the agent of change." But change is, doubtless, sometimes for the better.

Not to dwell, however, on speculations so transcendental, let us give Mr. Burke the commendation due to the ability with which he argues the question of differences of complexion as affected by external agencies. Here, as we have said, the present tendency of science is to revolt from the rule contended for by Prichard, that differences of temperature, local circumstances, and habit, are alone sufficient to account for the diversities of mankind in colour and stature. And, indeed, it seems hard to imagine that a European race, through any length of residence on the Senegal coast, or any length of habituation to savage life, could become Negroes in complexion, in figure, or in mental characteristics. And in point of fact there appear to have been races differing almost as widely as Europeans and Negroes, time out of mind, resident side by side in the hottest countries, pursuing lives of a like kind, and yet showing no tendency to approximate, save by intermixture of blood. It would almost seem as if, when God gave separate languages to men at the dispersion, and so divided them into different families, he had imparted at the same time separate characteristics of body and mind; and that, as by the fusion and mixture of a number of originally separate dialects, the prevalent languages of civilization have been formed, so by the mixture of bloods the traces of the aboriginal stocks are destined to disappear in the growth of a comparatively small number of dominant races;—all things appearing to tend to ultimate homogeneity. In the mean time whatever backward traces we can discover seem to point to past disparity, and to disparity of immemorial standing. In the Brazils, the hottest parts of the new world, white and the darkest copper-brown tribes dwell, and to all appearances have for ages dwelt, side by side. In Peru the nations of the torrid plain at the foot of the Andes are, Mr. Burke alleges, fairer than the dwellers on the table-land above; the latter, he asserts (but we know not on what authority), "being the very darkest of all the purely brown races of America." In Chili, he cites the Abbe Molina for the same

fact. In the Pacific Islands, the Polynesians and bearded Negroes offer a like example:

"Nothing can be more distinct than these two families of races, in all their peculiarities, mental and physical. The true Polynesian is gentle and friendly, gay and unsuspicious, of pleasing colour, handsome features, and fine form. The Negro or Cannibal, dark, suspicious, and treacherous, disagreeable in complexion, and frightful in features. Yet here are these races, living in the same latitudes, equally exposed to the vicissitudes of the weather, equally aboriginal, equally uncivilized, and yet thus continuing through thousands of years" [we must observe that here, as elsewhere, Mr. Burke, in the midst of much that is just and reasonable, lamentably damages his argument by palpable assumptions] "to differ in form, features, colour, language, and disposition, and never evincing the faintest approximation to each other in any of these particulars, except when it is evident that they are mixed in blood."

In the Indian Archipelago the Malay and Papoua races present similar diversities. Some of the fairest races of this part of the world are, it seems, on the authority of Mr. Crawford(*a*), to be found upon the very equator. The Javanese are among the darkest; the Dyaks of Borneo among the fairest. On the continent of Africa the Galla tribes are emphatically white Ethiopians. Many of the Moors of Barbary are as fair as Europeans; but may not this have arisen from an infusion of Scandinavian blood during the Norman kingdom? Amongst the retinue of Abd-El-Kader might be seen red hair and florid, though bronzed, countenances. In Southern Africa the Hottentots, Caffres, and Bosjesmans live under the same sun, seek their victuals, clothing, and shelter from the same plains and forests, yet in mind, body, and estate each differs from the others utterly, and, unless by intermixture of blood, irreconcilably. Major Harris, in his Travels in Central Africa, has made us acquainted with a tribe of pigmies dwelling in the south-west of Abyssinia, but three degrees north of the equator:

"They do not exceed four feet in height; their complexion is a dark olive, their hair is *not* woolly (that of the Bosjesman is so), and in the female reaches to the shoulders; the men have no beards, and they go perfectly naked; they have no king, no laws, no arts, no arms; possess neither flocks nor herds; are not hunters, do not cultivate the soil, but subsist entirely upon fruits, roots, mice, serpents, reptiles, ants, and honey; and all these things they eat raw, for they are ignorant of the use of fire."

Reading such an account as this, one reverts, with something

(*a*) History of the Indian Archipelago.

like a disposition to credence, to the traditions of the early discoverers of fire, the inventors of the plough, and the first devisers of laws, which are found in the records of almost every civilized country. But it seems a favourite theory just now that such improvements never originate spontaneously in any barbarous country. Granting, however, that the poor pigmies of whom we are now writing will, in all probability, have to look for their Prometheuses and Triptolemuses from amongst their neighbours, we see no reason for believing that these neighbours had in like manner to receive their first instruction in the rudiments of life from sources external to them; a conclusion which, if pushed to its legitimate extent, would end in referring all the arts of civilization ultimately to special divine communication. The niceties and varieties of grammatical inflexion found in the older languages, and the difficulty of supposing men in a state of barbarism capable of devising forms of speech so complex and scientific, forcibly confirm the averment of the Scriptures, that language, in its diversities, was so communicated: but when, in the absence of any authoritative warrant of that kind, the arts of kindling fire, cooking food, tilling the soil, committing sounds to written representative signs, and the thousand other processes of civilization, are referred to a like origin, the argument seems to be pushed beyond any foundation of reason or likelihood. But because a writer cannot imagine how mankind could have invented anything, is surely more a reason why he should distrust his own ability to form a judgment, than arrogantly adjudge that, *therefore*, the invention must have been specially communicated by miraculous interposition. We notice this dogma, of the incapacity of a race to elevate itself, the more particularly, because not only do we find it insisted on by Mr. Burke, but because it has been often brought forward as regards our own countrymen; many writers having attempted by most fallacious arguments to prove the hopelessness of the Irish ever being able to do anything good or useful for themselves;—a pernicious delusion, which has already done abundance of mischief, and, we fear, is destined to do an infinite deal more.

We might multiply examples from Mr. Burke's dissertation, *valeant quantum*, of the co-existence of striking physical disparities with location in the same region; but we are sensible that in all these the evidence of what is essential to the argument is wanting, namely, that the races with discrepant characteristics have been located in their respective places as long as the others; for, until this shall be established, no conclusion

can be drawn with certainty, however much the frequency of phenomena connected with such a state of facts may incline the mind to believe that such a state of facts may really have existed.

On the whole, we think this undertaking of Mr. Burke a meritorious one, and cannot deny him the credit due to diligence and ability; but his method is ambitious, his matter often insufficiently established, and his style more becoming a demonstrator than an inquirer. His pre-arranged classifications, unphilosophic and unnecessary in themselves, are destined besides to perplex his future acquisitions, and to deprive many a valuable fact of much of its usefulness by coercing it into a place not properly its own.

Still more transcendental and dogmatic are the speculations of Dr. Knox, whose lectures on the races of men occupy a series of numbers of the *Medical Times*, commencing with the publication of the 17th of June last. He is possessed with the idea of the distinction of races to an excessive degree, insisting that "no race will amalgamate with any other. They die out or seem slowly to be becoming extinct." This notion appears to have taken possession of his mind, with respect to the Sclavonic and Teutonic people of southern Germany, in an especially engrossing manner. The conflict now going on in Hungary, and which, while we write, has embraced Vienna itself in the widening circle of its influence, affords, however, a striking practical commentary in confirmation of these views; and it appears Dr. Knox does not now put forward his opinions on this subject for the first time:

"In my lectures delivered two years ago," he says, "when Europe was tranquil, I did not hesitate to foretell the coming approach of that great struggle between the dominant races for supremacy, now manifest to all thinking men, however the causes and the probable result may be mystified by a venal and a jesuitical press. Truths as manifest as the light of day had been either denied or shuffled aside in order to give a character of importance to the petty squabbles of financiers and statesmen. The language I then used respecting Germany and the Germans was considered by many as extravagant; and my statement respecting the comparative feebleness of the Austrian Government, and the causes of that feebleness, was declared to be contrary to the plainest observation.

"Again, when I stated that the Austrian empire existed merely by the sufferance of the other two great races—the Celtic and the Sarmatian—that it did not represent a *German empire*, properly so called, since most of its inhabitants were not Germans, but Sclavonians—a race with whom the true German would not amalgamate—

I found it difficult to make myself understood. The scholars of my country did not seem to know who the Sclavonians are, and could not see in them a *race* distinct from the German. Yet I foretold their disunion; showed that the Sclavonians had never amalgamated with the German; predicted that the Austrian empire would crumble to pieces if again attacked; explained as well as I could the downward course of the Sclavonian race, their disastrous defeats by the German and Celtic races, from Gustavus Adolphus to Napoleon. Let the events of the day speak for themselves. The long-headed statesmen of Europe despised the elements of race; the historian, the journalist, spoke of it merely as a philosophic theory, influencing neither the character of the nation nor of the individual. I pointed in my first lectures to Ireland, and was answered that the Irish had no doubt once been Celts, but, by living under Saxon laws, had now become very good Saxons! I was told that they (the Irish) were Catholic and spiritual, and believers in the faith, not because they are of Celtic blood, but merely by accident; that the English, Danes, Normans and Swedes, Hanoverians, Dutch, Saxons, were Protestants also, no doubt by accident, and not because they belong to the Saxon race of men. Thus making of all human history, as M. Guizot has done, a chapter of accidents. I next pointed to the Saxons in Europe—insular and continental, broken and dispersed; then to Celtic France—united, firm, and terrible to mankind, should she again take up the sword; to the jumble of races forming what was then called the kingdom of Prussia—Saxon, Sarmatian, Sclavonian; and to Austria, made up of tribes of Lithuania and of Southern Sclavonia; to the primitive Huns, Croats, and Germans, of the Baltic, held together by the sword. Overwhelming numbers of race (the Sarmatian or Russ), advancing as a vast wave of human beings towards Europe from the remotest steppes of Asia, are about once more to try their strength with the Celtic race—with civilized Europe. The Sarmatian and the Sclavonian races will, probably, unite to recover their lost position in Europe, to reconstruct the old falsehood, that the Sclavonians are Germans, and Vienna the capital of Germany. Some ten or twelve centuries ago the Gothic (Sclavonic race) seized the most fertile portions of the Roman empire,—called themselves Germans, sons of Teutonia!—established what they called the German empire. Vain attempt! Rejected by the true German, or the Saxon, as they ought to be called, the pretended head of all Germany now reluctantly throws off the mask, quitting the half-German, half-Sclavonic Vienna, and seeking a refuge amongst his Hunnish, Finnish, and Sclavonic tribes.

We cannot say that Dr. Knox's triumphant tone is not allowable, where events have so signally confirmed his predictions, made at a time when their confirmation in such a manner must have appeared extremely improbable. But, however happy his prognostications respecting the possible dissolution of the Slavo-Tudesque empire have been, he would be ra-

ther credulous who should expect similar confirmation of Dr. Knox's views respecting other races, which, we confess, appear to us to be at once extremely arbitrary and extremely visionary. But very little, indeed, save what is arbitrary and imaginary, could reasonably be expected from a writer who, in the present state of knowledge on these subjects, undertakes to settle and demonstrate

1st. The antiquity of races.

2nd. The theory of progress.

3rd. The theory of extinction and development.

4th. The theory of beauty of form, of perfection, and perfectibility.

And, lastly, the transcendental anatomy of all the past and present forms of animal life.

Compared with promises so ample, any performance to be expected from the best-informed writers of the age could hardly be otherwise than defective and unsatisfactory; but when Dr. Knox, in fulfilment of his undertaking, proceeds to confess that, regarding the Copts, the first race whose antiquity he undertakes to settle, he positively knows nothing at all, our expectation of enlightenment on transcendental anatomy and the other high matters proposed to be demonstrated, suffers extreme discouragement.—"A race I have not seen," —of whom, "whether ancient or modern, I can find only conflicting statements,"—who, in appearance, "in so far as I can learn, resembled the ancient Egyptians, judging of them by the busts still preserved, but even this fact I cannot fully make out,"—whose language "corresponds, I think, with the ancient Demotic,"—but "no one now thoroughly understands the hieroglyphics, and I doubt the accuracy of all the interpretations,"—and as to the historical evidences bearing on whom, "I look on the history of Josephus as, perhaps, the most monstrous historic exaggeration ever penned, and he as one of the greatest liars." Certainly these are not the data from which we can expect any sound conclusions; nor is Dr. Knox's the sort of investigation from which conclusions worthy of such data as really are accessible are likely to be derived. But, as we have already said of Mr. Burke's undertaking, the task, however we may quarrel with its execution, is, in its general scope, a meritorious one; and it would be unphilosophical and uncandid to deny credit to a labourer who is, at least, in earnest, and brings an active and vigorous mind to the prosecution of a favourite study.

We have placed Colonel Smith's book last in our list of subjects, as worthier, from its pretensions and execution, of

the deliberateness of last words. Colonel Smith is not inferior to either Mr. Burke or Dr. Knox in ambition, and exhibits, we think, an even greater degree of complacency in the disposal of his subject and the enunciation of his conclusions. Not satisfied with the distribution of the human species in their present seats, he enters on a preparatory review of the earth as designed for their first reception, and as modifying their subsequent progresses and ramifications by the successive changes of its surface and condition. We are thus presented with a preliminary treatise on physical geography, which, although very well executed, is *dehors* the immediate subject of Colonel Smith's inquiries, and would, we think, be more acceptable and useful in a separate form. Colonel Smith, an engineer, is possessed to excess with the geodetic idea. He examines the ground with a thousand unnecessary precautions, defining sea margins, river basins, mountain chains, and continental water-sheds, with a degree of precision sufficiently interesting to any one desiring a geographical survey of the globe, but, in bearing on the professed subject of his work, almost totally irrelevant. Yet we have rarely met with geographic description combining so much of truth and of preciseness; and would esteem this preliminary treatise, if enlarged into a separate volume, a valuable and acceptable addition to the physical library. We remember, however, that in this notice we have to deal with the living actor—man, not with the lifeless stage on which he plays his part; and must leave Colonel Smith's description of the *terrene* for notice in another place.

As regards man himself, Colonel Smith leans to the belief in a specific difference between the three typical stocks to whom the whole race are, in his opinion, referable. He demurs to the test of specific difference usually received by zoologists, viz.:

> "That the faculty of procreating a fertile offspring constitutes identity of species; and that all differences of structure and external appearance compatible therewith, are solely the effects resulting from variety of climate, food, or accident."

If this definition were true, Colonel Smith submits that the wolf, dhole, jackal, and dog, must be held to be one species, for, he asserts, their hybrids are fertile. So, also, he asserts, are the hybrids of the goat and sheep, of the hare and rabbit, and of the cow and zebra: *sed de his omnibus quære?* In the case of the cano-lupine hybrids reared by Cuvier, the breed, it is alleged, degenerated rapidly, exhibiting a continually decreas-

ing fecundity, which must have led to ultimate extinction. As to the other alleged instances of prolific hybrids, we could have desired references to some authority; but Colonel Smith neither alleges these matters of his own knowledge, nor adduces the testimony of others in support of his assertions. Simply, we do not believe the fact to be as alleged; but even though we discredit the alleged failure of the tests in these instances, and adhere to the first half of the formula as propounded in the above words, we are far from adopting the second proposition, viz., that all the differences of structure and appearance amongst those capable of procreating fertile offspring, are properly referable to varieties of climate, food, or accident. It seems impossible to believe that any variety of climate, food, or accident, could ever make of a Tahtar or Caucasian a Negro, or of a Negro a Tahtar or Caucasian. Something there must be of a more than accidental, though less than specific difference: just as, while no naturalist denies the specific identity of the bulldog and greyhound, the spaniel and the mastiff, the boldest theorist will hardly venture to allege that by any influences of air, of temperature, of location, or circumstance, either or any of them could be made to pass into the form or to assume the instincts or characters of any of the others. Be the original diversity of what origin it may,—whether specific, or whether of a character more consistent with an original parentage,—certain it is, at all events, that among men, as among the lower animals, and especially among that tribe of the lower animals which is most friendly to man, and most participates in man's higher attributes, there are well-marked *quasi*-specific differences, which no change of time, or place, or circumstance, has yet been found sufficient to alter, and which can only be modified, so far as we yet know, by the one method, of intermixture of blood. These stocks, as we have said, are regarded by Colonel Smith (as, indeed, we think, most inquirers now incline to regard them) as three in number, the Negro, the Tahtar, and the Caucasian; or, to employ more descriptive names, the Black, the Beardless, and the Bearded varieties. The desire to systematize (a form of the inductive faculty which may be almost called instinctive) at once suggests the reference of these three stocks to the three Noachian branches: but, however consistently we may refer the black races to the parentage of Ham, we cannot assign the beardless variety to either of the other Noachidæ, for the Semitic Jews and the Japhetic Scandinavians are alike bearded men. Indeed, when we look abroad into central Asia, and carry our contemplations into the regions beyond the Indus, we find that an adjust-

ment of the tripartite natural division to the tripartite Noachian descents increases in difficulty, till it seems hopelessly impracticable; and, as regards the progeny of Noah, we shall, probably, be obliged to reconcile ourselves to the following views of Schlegel, as perhaps the wisest and discreetest that can be propounded in the present condition of facts and of authority:

"Moses taught that man grew not up everywhere, and by chance, but was framed and fashioned by the hand of God himself, out of the earth, in one particular spot; and that the spark of divinity with which he is animated was not the fruit of robbery or audacity, but freely communicated to him by the love of his Maker. This doctrine affords the best clue to the history of man and that of his mind, and also the best point to which we may refer all the other traditions, and all the other doctrines of the East. According to it, the oldest dwelling of the human race, and the scene of their earliest development, lies in the middle Asia, between the Euphrates and the Tigris, the Gihon, the Ganges, and the South Sea: the present race of men are entirely separated from that early people by an universal catastrophe of natural desolation. The nations which have become again cultivated since this catastrophe may all be referred to three great families, remarkably distinguished from each other by their spirit and character. The first is one spread abroad, for the most part, in that same middle Asia, and from the earliest date more enlightened than the others. The second is a race, diffused principally over the north, of people more rude, indeed, but, at the same time, less corrupted and debauched in their manners, and, on that account, destined to derive, in after times, the chief benefit from the more early civilization of their eastern neighbours. The last, a race of men which had, indeed, a very early part in all higher knowledge and refinement, but sank, even in the oldest times, into unworthiness and neglect, from their fearful moral corruptions, and that mental bewildering and apathy to which these gave birth. This account of Moses is so confirmed to us by all the monuments and testimonies of antiquity to which we have access, is so extended and strengthened by every inquiry which we pursue, that it is well entitled to be viewed as the foundation of all historical truth."

Schlegel, here, in his contemplation of the Noachian progeny in its tripartite division of Semitic, Japhetic, and Hamite nations, manifestly confines his view to that part of the earth properly called the Old World, leaving the external races to be affiliated in some other manner to the original Adamite stock. In plain terms, such a classification seems not inconsistent with a belief in the continued existence of some of the antediluvian races; and if the necessities of science should at any time compel the inquirer to have recourse to such a solu-

tion of ethnographical difficulties, the example of so eminent and learned a Christian as Frederic Schlegel will, it may be hoped, serve as a sufficient protection. Indeed, the cautious and orthodox Prichard conveys much the same notion in the general result of his great work on the natural history of man, classifying under the head of the Nomadic races a large proportion of the human family to the apparent exclusion of the threefold progeny of Noah. A tabular digest of Dr. Prichard's arrangement will better illustrate what we mean to convey.

RACES OF MANKIND AS CLASSIFIED BY DR. PRICHARD.

1. *Syro-Arabian, or Semitic.*

Its branches are:—1st, Aramæan, or Syrian; 2nd, Hebrew; 3rd, Arabian; 4th, S. Arabian, or Ekhkili.

2. *Egyptian (Hamite).*

3. *Indo-European, Arian, or Japhetic.*

Its branches in Asia are:—1st, Hindoos; 2nd, Persians; 3rd, Afghans; 4th, Balúchi; 5th, Kurds; 6th, Armenians; 7th, Ossetines.

Its colonies in Europe:—1st, Celts; 2nd, Germans; 3rd, Lithuanians; 4th, Sclaves; 5th, Italians, excepting the Rasenians, or Etruscans, who probably emigrated previously; 6th, Thracian, or Skipetari; 7th, Hellenes.

The five great Nomadic races are:—1st, Ugrian; 2nd, Turkish; 3rd, Mongolian; 4th, Tungusian; 5th, Bhotiya.

The Ichthyophagi of Northern Asia.

1st, Namollos; 2nd, Tschutschi; 3rd, Kamtschatkans; 4th, Yukagers; 5th, Samoiedes; 6th, Kurilians.

The aboriginal Races of India.

1st, Singhalese; 2nd, Tamulian; 3rd, Parbatiga.

Races of the Caucasus.

1st, Abassians; 2nd, Caucasians; 3rd, Lesghi; 4th, Georgians.

African Races.

1st, Nubians; 2nd, Berbers; 3rd, Galla; 4th, Mandingos; 5th, Fúlahs; 6th, Hottentot and Bushmen; 7th, Kaffirs.

Races of Oceanica.

1st, Malayo-Polynesian; 2nd, Pelagian Negroes; 3rd, Alforas.

Races of America.

1st, Esquimaux; 2nd, Athapascas; 3rd, Algonquin-Lenápe; 4th, Iroquois; 5th, Cherokees; 6th, Sioux; 7th, Black tribes of California; 8th, Nootka-Colombians; 9th, Aztecas, or Mexicans, and Toltecas; 10th, Andian tribes of S. America; 11th, Brasilio-Guarani; 12th, Mediterranean group.

Looking at this table, it is difficult to avoid the conclusion, that Dr. Prichard takes the same general view which we conceive the extract above cited justifies us in ascribing to Schlegel. Colonel Smith, as we have seen, aims at a tripartite division still more absolute, in which no abnormal tribes have to be accounted for as nomadic or exceptional; but, while insisting on this threefold division, he discards, or, at least, tacitly disregards, the tripartite analogy of the scriptural account. He is compelled, however, to admit some exceptional races under the names (equally convenient as Dr. Prichard's "nomadics") of abnormal and aberrant types. Of these the giants and dwarfs of all races form one group, with which our author deals very agreeably. In a note to his chapter on giants are contained the following curious particulars:

"In the list, among the giant tribes of Syria alone, we find so many, that it is evident they were mere families, ruling, most likely by conquest, over Canaanitish tribes,—Nephilim, Rephaim, Zuzim, Gibborim, Enakim, Zamzumim,—some being distinguished by a malformation, having six fingers and six toes on the hands and feet; of which there is a counterpart in the legends of India. Of the stature individuals may have attained, are the examples of Teutobochus, king of the Cymbers, whose head overtopped the spears, bearing trophies, in the triumph of Marius. The emperor Maximinus exceeded eight feet; Gabarus, an Arabian, in the time of Claudius, was nine feet nine inches high; he was shown at Rome. In the reign of Augustus, Pusio and Secondilla were ten feet three inches in height, their bodies were preserved and shown in the Sallustian Gardens. The Emperor Andronicus was ten feet high, according to Nicetas. Herodus Hercules was eight feet. Porus, six feet nine inches. Charlemagne, seven feet. George Castriot, or Skanderbeg, and George Freunsberg, nearly eight feet. Without, therefore, vouching for the exact measurements here given, we have still sufficient evidence to show that, even in recent times, men of high stature and of immense strength have been historically conspicuous. The last trace, in Great Britain, of the giant character,

may be perceived in the *Broinech* of the Hebrides, where they were called *Gruagaichs*, Gruage feachd; a hairy bandit; concealed in the glens, and coming forth at night to plunder. During the operation of the Berlin and Milan decrees, we have personally known, in London, a Moor, usually named Gibraltar, captain of a neutral merchant ship, who was visible, at a great distance, in the Strand, head, breast, and shoulders, above the hats of the passing crowd; for he measured six feet seven inches and a quarter; and was, in all respects, of the finest proportions, and of very considerable acquirements in languages, &c."

We have already adverted to one pigmy nation of Abyssinia. Colonel Smith, in his chapter on dwarfs, supplies us with some further examples of the same kind, such as the Vedas of Ceylon, Cookies and Goands of Chittagong, east of the Bramahputra, who "reside in trees, with little more contrivance or use of reason than is evinced by the great apes of Africa."

"The Pouliahs of Malabar are no better; for they also form a kind of nests in trees, beyond the reach of elephants and tigers, never associating with other nations, and not even permitted by the Hindoos to approach within one hundred yards."

Giants and dwarfs, he shrewdly observes, are generally found in company; and, no doubt, these traditions do look very naturally for their origin to the exaggerated notions of force and bulk on the one hand, and of cunning and ingenuity, making up for want of strength, on the other, which would prevail in states of society composed of feeble slaves and fierce conquerors, the former being generally the aboriginal inhabitants, the latter, conquering invaders. In this way the traditions of Scandinavia respecting their dwarf smiths, builders, and miners, and giant warriors, seem plausibly enough referable to the Finnish aborigines and their Asian conquerors; the former supplying by ingenuity their want of physical strength, and possessing, probably, a greater knowledge of the arts of life than their masters. "These qualities," says our author, "in conjunction with retiring, defensive habits, have, in every region, conferred on such tribes the reputation of mystical properties." Hence we have, he suggests, the Peris and Genii of western Asia, the Elfin of Northern Europe, Scrclings, Brownies, and soforth. Respecting these feeble races in all parts of the world where they are commemorated in tradition, this is remarkable, that they are always represented as of swarthy complexions. On the other hand, whether from the design of heightening the contrast, or from traditionary evidence, the portraiture of the

conquering giants has been drawn, with an equally observable uniformity, in xanthous colours.

The next aberrant form noticed by Colonel Smith we should suppose even more questionable, that, namely, of the Aturian Paltas or Flatheads of South America, whose crania, he alleges, not only exhibit the natural type from which the artificial flat heads, formed by compression, are derived, but are furnished with a special "bone of a triangular shape between the two parietals, below the lambdoidal suture, separating the latter from the inferior margin of the squamous part of the afterhead." And this bone, it seems, having been observed in the occipital configuration of a certain infant of Peru, by Dr. Tschudi, has by him been designated the *Os Incæ*. But as remarked by, we believe, Dr. Scouler, in a learned and valuable notice of Colonel Smith's volume in a recent Number of the Dublin University Magazine, this *Os Incæ* of Dr. Tschudi is by no means peculiar to the children of the sun, being nothing more than one of the *Ossa Wormiana* largely developed, as it occasionally is in individuals of all races.

Leaving the children of the sun to seek a human parentage with the rest of Adam's offspring, and before passing to Colonel Smith's weightier conclusions, we would mention, amongst the other abnormal races noticed by him, the Cagots of the south-east of France, the supposed Saracenic stragglers of the Dauphiny, the Chuvash, a very degraded "semi-brute" population of Orenburg in Russia, the Ogres or Gholes of Rajahstan, the Samang dwarfs of the Malayan mountains, and various others, of whom the accounts are too imperfect to enable us to say more than that they are very weak, wretched, and obscure, and exhibit more of the characteristics of the Mongolic or beardless stock than of either of the other typical varieties.

Coming, however, to the main division of his subject, Colonel Smith fixes on three as the number of his typical stocks, because, he alleges, this division satisfies all the extremes of difference in stature and colour; and these three stocks, he insists, have received each a specific centre of existence, meaning by such centre a region of the world so proper to that race that none of the others can there predominate over it. The black nations, for example,

"Multiply on mountain and in morass, in sterile and in rich soil, throughout the tropical region. Though a new locality, like South America, be not their original centre of existence, they spread on both sides beyond the equatorial belt over the lower degrees of the

temperate latitudes; do not decrease in the province of the Caucasian when not overworked by their taskmasters; and flourish under the fiercest solar heat, when other types of men decay or perish. In constitution they escape or withstand many of the most virulent epidemics, amongst the rest small-pox, so fatal to all the American races; and others incidental to the tropics, or introduced by Europeans, visit them with less violence."

These considerations suggest, naturally enough, notwithstanding the allowance which must be made for the somewhat overcharged statements of a sanguine writer, that the equatorial region has been, in whole or in part, designed for the occupation and inhabitation of a black race, and give every appearance of probability to the theory, that within that region no superior European power will ever be able to supplant or exterminate them. Special centres of existence of a like kind for the other two typical stocks are more difficult of establishment; nor do we think Colonel Smith has succeeded in showing anything prohibitory of Caucasian inhabitation and propagation in the seats of the Mongolic families; while the descent of the Hunnish nations into Europe within comparatively modern times proves how easily the latter can adapt themselves to the climate and other conditions of the "centre of existence" of the Caucasian. We think the case stands confessed as regards the Negro; but to distinguish the Mongolian from the Caucasian, form, colour, and mental differences are the characteristics on which alone reliance can be placed. In his indication of these differences Colonel Smith writes very well and philosophically. Let us take, for example, the moral characteristics of the Mongolian or Hyperborean type:

"The Hyperborean evinces a feebler innervation than the other typical forms of man; he is less under amatory influences, less prolific, less enduring in toil, hence more disposed to severity where he has power; to a victim or a captive inflicting needless torture, less from natural ferocity than from the want of individual self-reliance, which is thus prone to express fear by precaution. More readily reduced to order when subdued; he evades rather than resists oppression by force; he is more obstinate than brave, but savage to self-destruction when roused by despair; avoiding personal exertion, such as to walk or to dig unremittingly in the fields, he rides in every region when the horse is accessible; more imitative than inventive, he exerts his ingenuity to apply mechanical aids in necessary labours; sitting at work he is dexterous, but little tasteful, at handicraft professions preferring patient elaboration to exertion; lazy, yet gluttonous, omnivorous with scarcely any distinction; filthy, amounting to a dread of water; crafty, dishonest, plausible; in war

he trusts to his horse or to numbers; he finds sudden irruption, cruelty, plunder, and desolation, more congenial than open battle and victory.

"With the mind more vacant than contemplative, the religious sentiment, that source of all exalted and practical feeling, has never risen above an indistinct idea of a Supreme Being, a heaven, or a solar worship: it is better satisfied with the true northern impostures of Shamanism, and with the borrowed demon worship engrafted on Budhistic doctrines; for what is of true moral tendency, either in the ethics of Foh or Budh, is of foreign origin, and repugnant to the intellectual puerilities which are his substitutes for reason, philosophy, and science. A deified, ancestral, and paternal obedience stands in lieu of practical religion—his only support that innate moral feeling belonging to all human beings. It is the key-stone of absolute power in the state; hence coercion is the civilization of the masses, ceremonious punctiliousness that of their superiors, ignorant self-laudation the acquirement of literati, and insolence the portion of all. The discoveries they possess in physics are the results of chance; all the maxims of state are immutable, and repressive of progress. Though early in possession of the mariner's compass, and (particularly the Japanese) long compelled to a familiarity with the sea, none of the beardless tribes ever became true navigators or reasoning ship-builders."

In a like indication of the Caucasian characteristics he is equally happy, combining an unusual precision of thought and expression with a high and feeling eloquence:

"The moral and intellectual character we find to be in unison with his structure: the reasoning powers outstripping the mere process of comparing sensations, and showing, in volition, more elevated thought, more reasoning, justice, and humanity; he alone of the races of mankind has produced examples of free and popular institutions, and his physical characteristics have maintained them in social life. By means of his logical intellect, he has arrived at ideas requisite for the acquisition of abstract truths; resorting to actual experiment, he fixed bases whereon to build demonstrable inferences, when the positive facts are not otherwise shown; he invented simple arbitrary characters to represent words and musical sounds; and a few signs, which, nevertheless, denote in their relative positions all the possible combinations of number and quantity; he has measured time and distance, making the sideral bodies unerring guides to mark locality and give nautical direction; he has ascended to the skies, descended into the deep, and mastered the powers of lightning. By mechanical researches, the bearded man has assuaged human toil, multiplied the results of industry, and created a velocity of locomotion superior to the flight of birds; by his chemical discoveries he has modified bodily pain, and produced numberless discoveries useful in medicine, in arts, and manufactures. He has found a sound and

connected system of sciences in general, and acquired a critical literature; while, for more than three thousand years, he has been the principal possessor of all human knowledge and the assertor of fixed laws. He has instituted all the great religious systems in the world, and to his stock has been vouchsafed the glory and the conditions of revelation."

On the characteristics of the Caucasian we need not dwell further; but we cannot pass, without remark, a fanciful argument for the supremacy of this race over the Mongolian, and of both over the Negro, as well as for the higher antiquity of the Negro race on earth, drawn from the alleged development of the human brain in the fœtus and infant. M. de Serres is the parent of this surmise. After tracing the fœtal brain, by a process with which the "Vestiges of Creation" have rendered even the popular reader sufficiently familiar, through the gradually developing convolutions of a fish, of a reptile, of a bird, and of a mammal, he proceeds to ascend in the scale by two or three other gradations, which we shall express with most gravity in his own words :

"But although in this progressive augmentation of organized parts, the full complement of the human brain is thus attained, the Caucasian form of man has still other transitions to undergo, before the complete *chef d'œuvre* of nature is perfected. Thus, *the human brain necessarily assumes the form of the Negro's, the Malay's, the American's, and the Mongolian's, before it attains the Caucasian.* Nay, more, *the face partakes of these alterations.* One of the earliest points where ossification commences is the lower jaw. This bone is, therefore, sooner completed than any other of the head, and acquires a prominence which it never loses in the Negro. During the soft, pliant state of the bones of the skull, the oblong form which they naturally assume approaches nearly the permanent shape of the American. At birth, the flattened face, and broad, smooth forehead of the infant, the position of the eyes, rather towards the sides of the head, and the widened space between, represent the Mongolian form, which in the Caucasian is not obliterated but by degrees, as the child advances to maturity."

If M. de Serres had said that the fœtal head successively attains to the *volume* of the Negro's, the Malay's, &c., before arriving at the development due to the Caucasian head, we might have been well enough inclined to assent; but what he alleges, implying an observable difference in form, rests on no foundation in fact, and its introduction, even in a note, by Colonel Smith, detracts from the character of his work. Structural difference there is none between the brain of one man

and of another; but the difference of volume is, unquestionably, very appreciable; and on this subject Colonel Smith supplies some observations which may be extracted with advantage:

"That the volume of brain is in relation to the intellectual faculties is clearly proved by Dr. Morton's researches, who, having filled for this purpose the cerebral chamber of skulls belonging to numerous specimens of the Caucasian, Mongolian, Malay, American, and Ethiopian (Negro) stock, with seeds of white pepper, found the first the most capacious, and the Ethiopian the smallest; though there may be some doubt whether the Negro crania that served for his experiment were not, in part at least, derived from slaves of the southern states of North America, who, being descended from mixed African tribes, and much more educated, have larger heads than new Negroes from the coast. We have personally witnessed the issue of military chacos (caps) to the 2nd West India Regiment, at the time when all the rank and file were bought out of slave ships, and the sergeants alone being in part white, men of colour, Negroes from North America, or born Creoles; and it was observed, that scarcely one fitted the heads of the privates excepting the two smallest sizes; in many cases robust men, of the standard height, required padding an inch and a half in thickness to fit their caps; while those of the non-commissioned officers were adjusted without any additional aid."

We have ourselves been informed by wholesale hatters, that different sizes of hats are required for different districts, even at home. Colonel Smith goes into further inquiries as to the comparative height, weight, and physical strength of the supposed types respectively. No adequate observations have yet been made on which to ground any certain conclusions in these respects. Assuming it, however, to be the fact that such typical forms of man exist, and have their respective centres of existence so assigned to them, it would appear reasonable to suppose that each should excel either of the others in its own special locality; thus, that a Negro regiment should outmarch a European one in central Africa, and a Tahtar horde endure the vicissitudes of the desert better than a Caucasian one. But we have the Caucasian caravans as patient of toil under the sun of Arabia as the dark nations of the Zahara under that of northern Africa; and the Anglo-Indian army actually excel the Sepoy troops in physical exertion in the climate of India. These speculations, therefore, grounded on the assumption of typical differences, can scarcely be pushed on any side to their conclusions without involving questionable matter. Still less, we think, are the observations which have been hitherto communicated to the public, respecting comparative weight, strength, &c., to be relied on, when

used to discriminate among the mixed sub-sections of individual nations. And we quite agree with Colonel Smith, that, as regards the alleged observations of Quetelet and Forbes, they are to be little depended on. The whole passage, however, is of interest:

"An instrument, the dynamometer, has been invented to measure the relative scale, and they have shown savage nations to be strong in proportion to the abundance and wholesomeness of the food they possess; but in all cases hitherto examined civilized Europeans surpassed them(*a*); and, it appears, English exceeded French; or perhaps more correctly, the Teutonic stock surpassed the Celtic, both in strength and weight, although the Irish Celts are said to be taller and heavier than the English Saxons. As yet, no great stress can be laid on results obtained from an imperfect instrument, partial inquiries, and questionable nationalities; still enough is determined to reject an opinion, often prevalent, that the moderns are degenerate when compared with their ancestors. The conclusion is further controverted by an experiment made at Goodrich Court, where the splendid collection of ancient armour is classified, with rigorous attention both to date and nation, by Sir Samuel R. Meyrick, the enlightened and munificent possessor. Two gentlemen, one of middle stature, with ample chest and shoulders, and the other somewhat taller, but of more slender structure, endeavoured to find armour sufficiently large to fit either one or the other, and failed, in a collection where, we believe, they had a choice of upwards of sixty complete suits of plate, all defensive armour, which, nevertheless, had been worn, in preceding centuries, by chivalry, and persons of distinction, in England, France, Germany, and Italy."

Passages such as the above, abounding in information agreeably conveyed, occur throughout Colonel Smith's work, and entitle it to the character of a useful and popular compilation; but, as an essay of scientific pretension, it wants caution and deliberation, and in point of style it is often loose, and not unfrequently inelegant. We are painfully sensible also, in opening any page, that we must encounter a greater or less number of assumed facts; and, by a fault very common among speculative writers, these assumptions are rarely put in the hypothetic form. Notwithstanding these imperfections, however, Colonel Smith's book conveys, within the compass of a moderate-sized octavo, and at a price accessible to the multitude, a vast amount of matter, put

(*a*) "The strongest North American Indians are asserted to fail against the ordinary power of wrist of Europeans; that is, when each side place the right elbow to elbow, and cross the fingers through each other's hand, striving to bend the opposing wrist back. The fact was established by the 60th Regiment in Canada."

before the reader invitingly, and calculated to direct intelligent minds to a higher contemplation of God through the noblest of his works. One true ethnographic conclusion, indeed, regarding man, as contradistinguished from the rest of creation, appears cheaply worth a whole library of formulas conversant with the tendencies of brute matter, or of zoological classifications of the forms of minor animals. It is thus that history, in its higher acceptation, embracing all the forms and arts of life, and dealing with the progress of a race of beings, the masters, so far as we know, of all creation here, and destined for immortality hereafter, claims a supereminent and paramount place among intellectual inquiries. The inducements to studies of this description are not, among us, such as they ought to be. The chief honours of learning are now bestowed on the investigation of laws intuitively discernible, and, when discerned, applicable only to the dead forms of things; laws admirable and sublime, but the knowledge of which gives no man a larger place in the heart or mind of his brother man. We would desire to see these pursuits made more worthy of the name of humane letters. To know how many legs an insect may possess, or the number of reticulations on the surface of a leaf, is knowledge, doubtless; but the greatest familiarity with the habits of the lower animals, and with the lower forms of life, cannot alone qualify man for conversation with man in the affairs of this wondrous existence in which we are placed. While, therefore, we perceive the necessity, for special purposes, particularly among medical men, of an intimate acquaintance with botany, zoology, and the other branches of physical science, we would wish to see the study of man himself, not only in his physical and mental organization, but in his past progress and coming destiny, promoted and prosecuted to a much greater extent than hitherto; and to that end would desire to see not only the aids of observation by weight and measurement and analysis, but the nobler aids of psychological science, of history, and antiquity, more generally resorted to by students in this branch of learning.

The fully-accomplished medical practitioner stands as it were in *meditullio scientiæ:* in the very centre of knowledge; for, since without health of mind there can be no health of body, and this holds true of the body politic as well as of individuals, the student of the life of man, in its manifestations of health and disease, is truly the student of the world over which man presides, and of the society which is made up of the aggregated minds and energies of men associated in their politic capacity. Yet how rarely is the physician given any voice in the gui-

dance of that machine of which he is to so great an extent the conservator, and of whose various parts he, of all others, if a true master of his science, possesses the most comprehensive knowledge? Even on this subject of ethnology, irrelevant as at first sight it may seem to the affairs of every-day life which surround us, the voice of the physician, if attended to amid the din of politics, might be found to convey warnings which the statesmen of our day would do well to bear in mind in the course of their policy.

But there is, unquestionably, another influence, besides that of climate, which operates in modifying the physical and moral man,—the influence, namely, of *country*. What shall be considered a country in this respect is a question of extreme consequence to the welfare of mankind; for all attempts to denationalize such peculiar regions are against the natural plan of the world, and result in social disaffection and unhappiness. Thus there can be no doubt that the efforts of Austria to Teutonize Italy have been and must ever be out of harmony with the nature of the Italian constitution, from whatever source the population of that country may be derived; for it is incident to that region to produce a population having a peculiar national character of its own. And the influence is not confined to merely moral characteristics. The Goth or Sarmatian, located south of the Alps, acquires a new aspect; as does he located south of the Pyrenees. So, in even a more remarkable way, the American settler, no matter from what race derived, who fixes himself anywhere between the Mississipi and the Atlantic, undergoes a positive change of form and feature, as well as new habits of mind; and his offspring, born and educated anywhere throughout that tract, will be distinguishable from their kindred in the mother country, both in aspect and in moral and mental habits. It appears also at least doubtful whether a similar specific influence is not exerted on the inhabitants of our own island, by something altogether independent of social contagion. For 600 years the experiment of assimilation between Great Britain and Ireland has been in course of trial; and, although up to a certain point it has proved successful,—though the Irish have accepted the language and social system of the more powerful island,—it may still be greatly doubted whether they ever will lose the peculiarities of thought and demeanour which to this day undoubtedly characterize and distinguish them. Whether there be any virtue in the formulas of social science, sufficient to remove these differences, it would perhaps be unphilosophic to deny; but it appears sufficiently clear that, in the other cases

we have referred to, Nature is not to be put out of her way by any devices of government or legislation; and the reasonable probability is, that here, too, she has decreed some characteristics to be unchangeable and perpetual.

But though the influence of climate, soil, and other such circumstances, must be allowed to exert a specific effect on the human subject,—and up to this point the theory of Prichard, and the other advocates of original identity of race, cannot with candour be denied,—these effects fall very far short of the typical differences which distinguish the great families of mankind from one another. It is observable, also, that such modifying influences operate much more conspicuously on some races than on others. A North American inhabitation of two generations makes little change in a Negro; but an Anglo-Saxon, under such circumstances, undergoes a palpable metamorphosis in figure, in feature, and in mind. Indeed, the Englishman of the present day seems the most susceptible of such influences of the whole European family. Here, in Ireland, he Hibernicizes more readily than the Irishman becomes Britannicised in England. It may be regretted; but the fact, however we may wish it otherwise, presents itself continually to our notice, and challenges the most serious consideration of those who project national reformations by the introduction of new blood.

This quality of the place, if such a quality do indeed exist, may be likened to the singularities of soil, where we often find the products of one district clearly distinguishable from the like products of an adjoining one, though no nicety of chemical tests can detect any difference in its composition. One vineyard yields wine of a peculiar flavour, differing from the vintage of the adjoining one. One farm yields cheese of a quite different quality from that of another; yet between the two is no apparent difference of soil or grasses; and the same wind blows and the same sun shines over both. If our surmise be true, as respects the human product, it would seem that Nature has appointed centres of nationality for the respective branches of each typical stock, as well as more enlarged centres of existence for the stocks themselves. If this, in even a modified way, be so, we repeat, it behoves the rulers of nations to consider well how they attempt anything against Nature.

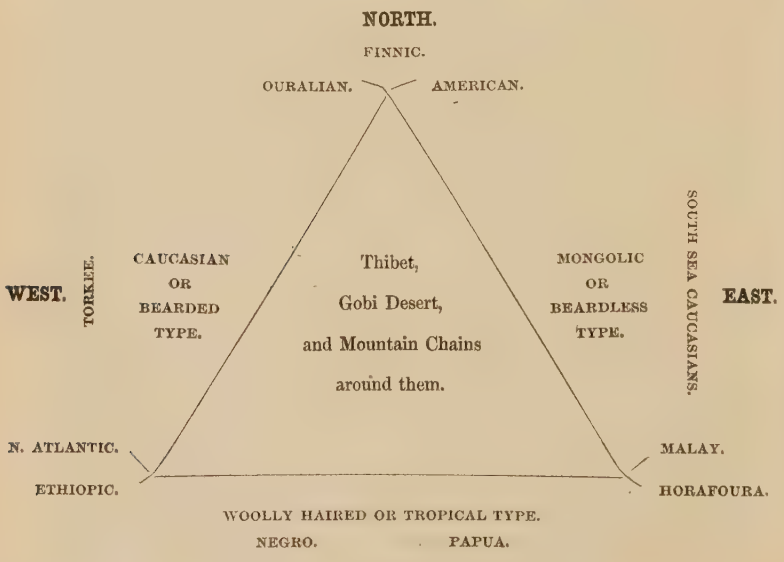
But as these speculations are among the highest in which the human mind can be engaged, so are they the most difficult. At every step the philosophic investigator must pause and examine his foundations. Rash footsteps here, more than anywhere else, are sure to plunge the wanderer in inex-

tricable error. There are no falser lights, to men engaged in such inquiries, than those held out by spurious etymologies. We see men of good ability and large acquisitions continually misled by the wildest assumptions in this department. See how Colonel Smith stumbles at once into the midst of a morass of follies by taking up with the etymologies of Goropius :

"The Pelasgians left also colonies at Norba, and amongst the Volsci, Hernici, Marsi, and Sabini, tribes having all names and characteristics of a Getic infusion in their dialects, and indications which show, like the first-named in particular, affinity with the Belgic Gauls, chiefly with the Volsci, Tectosages, and Aurunci. The word Volsci, Velka, Wilki, or Teutonic Volka, is generical for "people," and the different tribes had each a particular designation. That of Italy was known by the appellation of Aurunci, from *Awe*, the vale or open country."

We recur involuntarily to Martinus Scriblerus, and the poached eggs of Alexander the Great.

To return, however, to Colonel Smith's general distribution of his subject. We have given an abstract of the arrangement adopted by Dr. Prichard. His classification is more convenient—more consistent, in truth, with our little knowledge—than symmetrical. Colonel Smith aims at a more absolute arrangement, and exhibits his notion of the subject in a diagram here shewn:



Into the details of the various offsets and affiliations proceeding from these supposed types, and their respective issues, we will not now enter. Colonel Smith, strongly impressed with the idea of centres of existence, deals ingeniously, and, we think, in any case, not uninstructively, with the reciprocal effects of type upon type, and type upon sub-type, where the primitive races overlap and interfuse with one another, or with one another's modified issues. In the midst of much that is speculative, and, doubtless, fanciful, we observe some facts of interest. Let us take one. The Negro receiving the Mongolian(?) innervation (as Colonel Smith expresses it) has a tendency to throw out the hair in a frizzy growth of extraordinary abundance, as exemplified in two portraits of men of this breed, taken (as most of Colonel Smith's illustrative portraits are) from the work of the late Prichard. Crossed by the Caucasian, the Negro's short, woolly locks subside and lengthen into a lanker growth. Are like phenomena observable in other intermixtures?

As we have said, it is not our intention, at present, to go farther than exhibit a general view of the system presented in Colonel Smith's work. As a work of science, it must rank far below that of Dr. Prichard; yet it deals with the subject more comprehensively, and will probably exert a greater influence in directing other minds into the same channels of research.

And now to descend from speculations, which, whether ill or well prosecuted, lead, at least, to regions more lofty than most others in which the human mind can expatiate, to a very ordinary and commonplace consideration. Mr. Lizars, Colonel Smith's publisher, has adopted a method of binding for his work which we earnestly wish were more generally used. The leaves are cut,—a great facilitation to perusal; but it wants an index,—a great and crying defect. When Curll summoned his authors to take counsel how to overthrow Pope, the index-maker came with the rest. They would have been a helpless corps without their index-maker. A great book of literature of the sixteenth or seventeeth century would have been held imperfect and deformed if it had not its Index Onomasticus, its Index Topographicus, and its Index Moralis, at the least. Writers aiming at the consolidation and systematizing of all past learning, now give us their volumes without any. But Colonel Smith and Mr. Lizars must be apprised that, however a critic may think it is his duty to read through a book, that he may pass an honest judgment on it, his judgment would be better given, and of more service, if he had the means of collating and comparing, which a good index alone can confer.

On the Treatment of Ulcers of the Leg without Confinement, with an Inquiry into the best Mode of effecting the permanent Cure of Varicose Veins. By Henry T. Chapman, F. R. C. S., &c., &c. London, Churchill. 1848. pp. 156.

A Treatise on the Cure of Ulcers by Fumigation. By George Alfred Walker, Surgeon, &c. &c. London, Longman. 1847. pp. 112.

Mr. Chapman states in the commencement of his essay, that a great portion of the contents of this volume was written upwards of four years ago, having been intended to form part of the article on Ulcers in the Cyclopædia of Surgery, and laid aside when that work was discontinued. His subsequent experience of the treatment advocated having strengthened the conviction he then entertained of its efficacy, the Author encourages the hope that the subject matter may be deemed to possess a sufficiently practical character to justify its separate publication. In the introductory observations he observes, that the inconvenience attending ulcers of the leg, when treated by means requiring confinement, their prevalence among those classes of society whose very existence depends on a free and vigorous use of their limbs, coupled with the impracticability that not much more than one-fourth of them can be admitted into hospital for treatment, appear to him sufficient motives for calling attention to any plan not *requiring confinement*, which, even if *not* more successful than those hitherto employed, possesses a decided superiority in point of simplicity, cheapness of material, and ready adaptation to the numberless modifications of the disease presented to our notice.

The peculiar characters which distinguish ulcers of the leg occupy the first part of the essay, and the conclusion arrived at is,

> "That one trait is common to them all,—a sluggishness or indisposition to heal under treatment, which ordinarily proves successful elsewhere. It is from this circumstance, and the degree to which they cripple active exertion that they derive their importance; it is this peculiarity, whatever may have been their origin, however various their aspect, which has attracted the attention of surgical writers from the earliest period, and has caused them to be treated of almost as a distinct branch of surgery."

Adopting the definition of ulcers as "suppurating sores which manifest little or no disposition to cover themselves with a cicatrix, it is to discover the sources of this intractability that the attention of the surgeon must be directed, in order to enable

him to decide with confidence upon the treatment best adapted to overcome any given case."

In the next chapter the sources of the intractability of ulcers of the leg are dwelt on, the Author ascribing much to the dependent position of the lower extremity, where the heart and arteries have to contend against gravitation of blood in the veins, and where the circulation, even in health, is scarcely carried on as in other parts of the body, which seems to be proved by the frequent occurrence of varix in these veins; the capillary vessels are accordingly placed under most unfavourable circumstances for conducting the process of regeneration. Mr. Chapman conceives that sufficient importance does not appear to have been accorded by any author to the circumstances in which the capillaries of a granulating ulcer in a depending part are placed by the loss of an elastic envelope like the skin. Nevertheless, although the local obstruction to the circulation caused by the depending position is a principal cause of intractability in the healing of ulcers of the leg, yet the fact that they do not always heal when this is removed proves that other causes may be superadded, viz., constitutional; and, in order to simplify the subject with regard to treatment, the Author divides ulcers of the leg into,

1st. Ulcers intractable from constitutional causes.

2nd. Ulcers intractable from local causes.

In the chapter on the treatment of the constitutional sources of intractability, we find much useful information collected, but as there is nothing novel contained in it, we proceed at once to the chapter on the local treatment of ulcers of the leg, to which the author points with so much confidence.

The local means of treatment on which Mr. Chapman relies are pressure applied to the limb by means of properly adjusted bandages, together with water dressings; but as the author considers that the benefit to be derived from these measures will depend on the manner in which they are applied, we must allow him to speak for himself on the subject:

"In the employment of cold water as a topical remedy in ulcers, at least an equal degree of caution is necessary to avoid the application of too low a degree of temperature, not alone to prevent the extreme consequence which may ensue, the extinction of all vitality in the newly formed structure, but to guard against the vitality being reduced to a degree incompatible with vigorous reparative action. Evaporation ought, therefore, to be controlled, as in irrigation, by enveloping the affected part of the limb in a sheet of oiled silk.

"There is not, however, in these cases, the same liability to reaction, from the irregular action of cold, nor the same mischief to be

apprehended from its occurrence, as in inflammation; on the contrary so far from being injurious to the progress of the ulcer, I am persuaded that the reaction excited by the renewal of the cold application is a beneficial stimulus to indolent and callous ulcers; and as the oiled silk is rather calculated to foster this reaction, at the same time that it guarantees the part from all undue reduction of the temperature, I never employ cold in those cases without directing the patient to cover the dressings with an oiled silk envelope.

"As soon, therefore, as all inflammatory action has been subdued by emollient and antiphlogistic measures, these may be advantageously exchanged for cold water dressing and support, applied in the following manner. The sore being dressed with a compress of lint dipped in cold water, folded once, twice, or three times, according to the degree of compression which the surgeon may deem suitable to the case, three or more moistened strips of linen or calico, about two inches and a half in width, are to be carried smoothly round the leg. These strips must be applied precisely in the same manner as the strapping recommended by Mr. Baynton; the middle of the first strip being placed upon the back of the leg, with its upper edge opposite to the lower margin of the ulcer, the ends are brought round to the front, drawn firmly, and laid down smoothly, one over the other; the second strip must cover the upper third of the first, and the same proceeding must be followed with as many strips as the size of the ulcer may require. Over the strips a calico bandage is applied, the greatest attention being paid to its equable adjustment, so that the compression be distributed evenly over the entire surface, and its amount regulated by the sensibility of the ulcer. Where the leg is slender, above the ankle, the roller is apt to fall into plaits, and furrow the skin; to guard against this inconvenience the hollows immediately above the malleoli should be filled up with compresses of lint. The whole is then to be soaked with cold water, and the moistened bandage enveloped with a sheet of oiled silk, reopening it from time to time to renew the cold affusion. The wet strips of linen adhere to the limb, even before the application of the bandage, almost as closely as adhesive strapping, and are capable of affording a support scarcely inferior to that derived from it. If the ulcer secretes abundantly, it is better at first to repeat the dressings daily, although the lint will absorb much of the discharge. Very shortly, however, under the action of cold, large, shining, semi-transparent granulations become compact and red, and a thin and copious secretion diminishes in amount, and improves in quality, rendering a daily renewal of the dressings quite unnecessary; and after a time this necessity becomes still more rare. In several of the cases hereafter recorded, an interval of three, four, or even five days sometimes elapsed between each dressing, without any interruption to the onward progress of the ulcer."

The patient during this treatment is allowed to enjoy moderate exercise. When the ulcer is very deep Mr. Chapman fills the

bottom of it lightly with shreds of soft sponge soaked in water, and over these applies the water dressings and roller as before. By many surgeons the author admits that water dressings have been applied, in conjunction with the bandage, to the treatment of indolent and callous ulcers of the leg; trifling, however, as the difference may appear between the modifications described above and the ordinary form of water dressings, the difference of effect, according to his experience, is very considerable. He believes it to possess the advantages without the disadvantages of Mr. Baynton's method. It requires to be renewed less frequently, and he has employed it in preference to all other local treatment in large indolent ulcers, when, from excessive irritability, the sore would not either tolerate cold nor the lowest grade of compression. Mr. Chapman does not, however (it is but right to mention), throw over board other means of treatment, but has recourse to them on particular cases, occasionally using Baynton's strappings, nitrate of silver, solutions of the various astringents, chloride of lime, &c., so as to meet the exigencies of each case; the compresses of lint used in water-dressing being dipped into the solution, and used in the same manner as in the cold water dressing.

In ulcers of the leg in a state of morbid sensibility, the result of the actual lesion of the nerves, as also in ulcers in individuals whose general vitality is low, the Author substitutes tepid water dressings and tepid affusion instead of the cold water dressing.

In alluding to indolent ulcers complicated with a varicose condition of the veins, Mr. Chapman states, that it is against the diseased condition of the veins that the surgeon's efforts must be chiefly directed; and observes that, if the radical cure of the varices cannot be attempted, the only effective palliative by which their injurious influence over the sore can be counteracted is the employment of pressure, either by medium of a tight bandage and compresses along the enlarged veins, or by applying strapping to the limb, as practised by Mr. Scott. But Mr. Chapman seems to be perfectly ignorant of the plan of treatment proposed by the late Professor Colles of this city, and found so useful as a palliative mode of treatment of varicose veins, viz., the vein truss, now so well known in this country as "Colles's Vein Truss." This omission is the less excusable on the part of Mr. Chapman, who professes to come before his brethren with all the recent information on the subject before us, inasmuch as the advantages of Mr. Colles's instrument have been placed before the profession in the first volume of the lectures of that eminent surgeon, which have

been recently published. The following observations we have taken from our notes of a lecture delivered on the subject of varix by the late Mr. Colles. After some lengthened remarks on the danger and failure of the different modes of treatment proposed for the radical cure of varicose veins, he says:

"As it appeared to me that the grand object in the treatment of varicose veins was to obliterate the caliber of the vein, it struck me that this object could be attained by means of simple pressure. I tried at first pressure on the vein at the inside of the knee, or a little lower down, but this was attended with much pain. I then tried pressure where the saphena vein dips down to join the femoral vein in the groin, and I found it to answer all my purposes. In these cases I now employ a small truss called the 'vein truss,' shaped like that for inguinal hernia, but of course much smaller; the pad is applied to the trunk of the vein, the strap being buckled round the buttock: you will find it very difficult to get the patient to keep the truss on the right place at first, but this we can prevent by marking out the proper spot for him. This plan has succeeded admirably with me, and the more I use it the more I am convinced of its utility: it need not be worn at night. This instrument is also applicable to pregnant women with varicose veins, who are subject to bleedings from these veins. I have seen some of these patients quite exsanguinous from the loss of blood in this way, and, by the application of the vein truss, the bleeding was arrested; when delivery takes place they may put the instrument aside."

In concluding our remarks on the chapter devoted to the treatment of ulcers of the leg, we must, in justice to Mr. Chapman, say that we have read it with much pleasure; and although the author does not assume to himself credit for originality in proposing water dressings and compression to certain forms of ulcers in the leg, yet we are of opinion that he has done much by directing the attention of the profession, especially the junior members, to the advantages of this combined treatment, a practice we have been familiar with for years, but which we have not applied precisely as recommended by Mr. Chapman. However, since we first read the essay before us, we have used the compress, straps of linen, and roller, adjusted as directed by Mr. Chapman, in two cases of ulcers of the leg, and we have found them to answer admirably well.

In the chapter on the *Radical Treatment of Varix* a review is given of the different operations which have been proposed for the permanent cure of varicose veins, together with the objections to which most of them are liable; and, after a brief allusion to the treatment by ligature, Brodie's method by subcutaneous incision, Velpeau and Jobert's treatment by pins and

twisted suture, and the treatment by caustic, Mr. Chapman proceeds to notice the method of cure by galvano-puncture, and says:

"Since the possibility of coagulating blood circulating in an aneurismal sac by the medium of galvanism has been established, the same principle has been extended by Italian surgeons to the cure of this disease (varix) of the vascular system."

The author then gives an account of MM. Bertani's and Lamberini's experience on the subject, but he has had no practical experience himself in the matter. We need not give the details of the cases reported in favour of this treatment, but the Author in conclusion very judiciously remarks: "Whether the cure is more permanent than by the other modes of obliteration remains still to be established, since no direct evidence has yet been adduced on this point." In fact the information we at present possess on this subject is anything but satisfactory. Its advocates state that it obliterates the vessel, and is free from danger; but the well-known fact of the return of the disease after the apparent obliteration of the vessel by other means obliges us to withhold our opinion, until the superiority of the galvano-puncture over all other methods of treating varix is satisfactorily proved by the permanency of its effects.

From the perusal of Mr. Walker's book we have derived but little useful information. The author relies on the application of the fumes of sulphur and iodine to the surface of the ulcer, and adjacent tissues, through means of an apparatus which he has constructed for the purpose. The apparatus consists of a series of mahogany boxes, having in the cover a round aperture for the reception of the limb, each box communicating below with the engine room of *his establishment.*

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1. *Contributions to the Pathology of the Kidney.* By William T. Gairdner, M. D. Edinburgh, Sutherland and Knox. 1848. 8vo. pp. 54.

2. *Transactions of the Royal Medico-Chirurgical Society of London.* Volumes XXIX. and XXX. 8vo. 1846–7.

3. *Cyclopædia of Anatomy and Physiology.* Edited by Robert B. Todd, M. D., F. R. S., &c. &c. London, 1848. Part XXXII. Article, Ren.

Our Readers may remember our recent promise to give some account of the remarkable investigations which have been lately

so successfully carried on in London and Edinburgh into the minute pathology of that ill-understood organ, the kidney. That promise it is the object of this article to redeem.

Great as the actual fruits of these investigations have already been, we have good reasons for considering them but as the commencement of a series of researches, the tendency and sure result of which will be the clearing up of one of the most obscure recesses in pathology. We believe the author of the article "Ren" in the Cyclopædia of Anatomy, upon which we have already bestowed high praise, has not spoken too sanguinely in "predicting that, within a very short space of time, the diseases of the kidney will be more completely and generally understood, with reference to their pathology, diagnosis, and treatment, than those of any other organ."—p. 263. It is worth while to inquire the grounds upon which Dr. Johnson has founded this prediction; they are:—1st, that there is no important organ in the body whose minute structure is so well known as that of the kidney; 2nd, that the products of disease in that organ are brought more completely under our daily observation than those of any other, inasmuch as its morbid deposits and accumulations occur, almost without exception, in such a situation that portions of these materials are being continually washed out by the passage of the fluid secretion over the canals in which they are contained. These form a good basis upon which we may safely build our hopes and predictions of greater enlightenment in these pathological obscurities. Yet how little does the vast mass of medical practitioners appreciate the importance of an accurate knowledge of the minute structure of the kidney, or of any other organ! How rarely do we see anything approaching a careful examination of the urine or any other secretion! Mayhap we may find, here and there, an hospital physician examining the urine for albumen, and this is about the sum total of his examination, as if pus, epithelium, mucus, spermatozoa, tube-casts, oil granules, and blood corpuscles, were pleasant fictions, about which a Gairdner or a Johnson may amuse learned societies, but which concern not the practical man. But this is not more nor less than a serious error, a criminal neglect of those resources which the advance of science has brought to the aid of the physician for the discrimination of disease. We know there are men, and such men will be always met with, who laugh at such things as "silly minutiæ," "refining too much," "making distinctions which nature has not made," and so forth; but we can assure the objector in good sooth, that it is no trifle to be *quite certain*

that your patient has spermatic discharges; that it is no very unimportant refinement in knowledge to be aware that your scarlatina patient is passing undeveloped epithelium before the manifestation of the slightest derangement in the general system; that it is somewhat satisfactory to be able to assure a sufferer that he is *not* labouring under what, in our present state of advancement, appears to be a necessarily fatal disease, but under one in which a cure is possible. These are the solid benefits of the *new* pathology, so much sneered at by the *soi disant* pathologists of an obsolete school, but which has already reclaimed many a sterile tract in long uncultivated fields; benefits which, permanent advantages in themselves, have the still greater merit of being *essentially reproductive.* Scientific discoveries are the producers of further scientific discoveries. The cell of the Bovista giganteum not more surely or more rapidly begets its like, than a great advance in science begets similar great advances. This is almost a truism. How easy and natural the transition from Franklin to Faraday, from Laennec to Stokes! We know that to the medical trader, the man who considers his duty performed when the fee is pocketed, the facts we are about to bring before our readers will be mere curiosities, all very well to be sought after and talked about by medical virtuosos, but by no means of general practical utility. To these we can only say, our duty is performed. We, in our critical and not *irresponsible* capacity, have inducted them into the choice wares of the science they profess to cultivate. The crime of their rejection be on their own heads.

In the publication we have placed at the head of this article, Dr. Gairdner has made his first appearance as an author. It is not too much to say that it is one of the most important monographs on the special pathology of an organ we possess. It is an invaluable addition to medical science, and reflects the utmost credit not merely on its author, but on the great and rising pathological school of which Dr. Gairdner(*a*) is already so distinguished a member. As we shall have very frequent occasion to bring this essay before our readers in the course of our analysis, we shall now proceed to enumerate the remaining sources from whence our facts will be drawn. First in point of time and intrinsic importance, is the paper by Dr. Johnson in the twenty-ninth volume of the Medico-Chirurgical Transactions, entitled, "On the minute Anatomy and Pathology of Bright's Dis-

(*a*) We are glad to find that, by the recent appointment of this gentleman as pathologist to the Royal Infirmary, he will have extensive opportunities of prosecuting his researches.

case of the Kidney, and on the Relation of the renal Disease to those Diseases of the Liver, Heart, and Arteries with which it is commonly associated." There can be no doubt that this now celebrated paper has effected an important change in our views of the intimate nature of Bright's disease, and that to it mainly are to be attributed the careful researches of succeeding writers; nor are we about to quarrel with Dr. Johnson that he was anticipated in Germany, as far as his chief facts were concerned. There can be no doubt that he was ignorant of what had been done by the German workers in the same field of labour, and who, by the way, appear determined to *do everything* in the pathological field; and equally, we presume, there can be little doubt of the truth of his own assertion, "that his views differ essentially from those of any preceding observer." In subsequently placing to his paper an appendix, in which he indicated the various productions of the German pathologists bearing upon his own observations, Dr. Johnson made a concession to the critics as graceful as it was politic. Though we may be allowed to state our surprise that the Royal Medico-Chirurgical Society of London—the medical St. Stephen's—contained no member on the night of June the 8th, 1845, who had heard of Gluge of Brussels, or seen his renowned "*Atlas der Pathologischen Anatomie.*"

In the same volume we find a paper from the well-known pen of Mr. Toynbee. Of this we shall have but little to say. It certainly does not sustain the character acquired by that gentleman for his early investigations into minute structure. All who have succeeded him disagree with the views propounded in the paper, and we fully believe he is equally at issue with nature,—here, as elsewhere, a fatal antagonist.

In the thirtieth volume there is a paper by Mr. Simon, the accomplished author of the "Essay on the Thymus Gland," on "Subacute Inflammation of the Kidney." This is a truly original and beautiful paper. It is rare, indeed, to find elegant scholarship and profound pathological attainments united in the same individual so richly as in this gentleman. We are as much charmed as surprised to find the abstrusest parts of what we have been accustomed to term *dry* pathology handled in so original and agreeable a manner. We are aware that Mr. Simon has been recommended by some critics to prune his ornate and luxuriant style. We, on the other hand, would advise him still to present us with deep pathology treated with something approaching literary taste,—a rare merit these days.

Immediately following Mr. Simon's paper we find another by Dr. Johnson, "On the inflammatory Diseases of the Kidney,"

which agrees in its main features with that of his then colleague, Mr. Simon; the latter circumstance, perhaps, accounts for their similarity, but they differ in many important particulars. In the thirty-second part of the Cyclopædia is contained the conclusion of Dr. Johnson's elaborate article on the kidney, which we brought before the notice of our readers in our last Number. This, the pathological portion of it, contains a somewhat sketchy *resumè* of the author's researches, contained in the two papers to which we have just referred.

We shall take Dr. Gairdner's essay as the basis of our observations, not merely because it gives the most complete account of the entire pathology of the kidney, but because, from the systematic arrangement adopted, it will enable us to bring the various facts bearing upon this hitherto obscure subject more conveniently before our readers; though we shall have constant occasion to refer to his collaborateurs in the same pathological field.

The essay opens with an excellent though brief account of the vascular system, tubuli, and parenchyma of the kidney. The condition of the vascular system has been minutely described by the earlier investigators of Bright's disease; but it must be confessed that their researches conduced but little to a satisfactory knowledge of the complex conditions implied in that name. Indeed, as Dr. Johnson has well remarked, this want of success may, perhaps, be attributed to the fact that the investigations referred to were made "with a certain preconceived notion on the subject, which has induced them to direct their attention too exclusively to the vascular system of the gland, while they have neglected the study of other portions of the organ, which, there seems reason to believe, are primarily and essentially concerned in the disease"(*a*). Yet it is certain that a right apprehension of the true condition of the vascular system is quite necessary as a preliminary to *all* ideas on this subject. We think the exact description of Dr. Gairdner will go far towards giving a somewhat definite notion of this part of kidney pathology, and tend to take away Mr. Simon's reproach, that "slight congestion" of the kidney" is the vaguest of all vague records which the case-book contains. It is Dr. Gairdner's opinion that the abnormal character of the vascularity is most important, as leading us to detect disease of the organ. The abnormal character may exist in the venous or in the arterial system, and in either case has certain distinctive marks, whereby the particular system implicated may be discriminated.

(*a*) Med. Chir. Trans., vol. xxix. p. 2.

The veins are disposed chiefly in two situations, viz., on the surface, and in the substance of the pyramids. On the surface they form a peculiar network, visible with the naked eye, first minutely described by Ferrein, and to which Drs. Gairdner and Johnson have paid particular attention. The general distribution of these veins is *stellate*, enclosing by their intersections small pentagonal spaces, in the interior of which the natural pale colour of the cortical substance appears, about the size of a very small pin's head. The condition of this superficial venous plexus varies greatly with the degree of fulness of the general venous system; it may, therefore, be perfectly anemic, or, on the other hand, in the highest degree of distention, in correspondence with such variations in that system. So far we have seen nothing to indicate a *special* state of the kidney; but Dr. Gairdner goes on to remark, that

"Great irregularity of injection, amounting to *marbling* of the surface, and great increase in the size of the stellar vessels, are generally tokens of disease, as they are the result either of partial obliteration of the venous network, or of the extrusion of the blood from it, through over-distension of the loops of tubuli which form the intervening pale spaces."—p. 3.

This is an important observation, which should be impressed upon the mind of the anatomist. While there are certain general conditions, not to speak of mere gravitation, which greatly affect the appearance of the kidney as a vascular organ, the irregularities described by Dr. Gairdner indicate special states of the utmost importance.

Our author next proceeds to describe "the engorgement of the capillaries and Malpighian tufts," which gives rise to two conditions: first, a generally diffused heightened colour of the cortical substance; and, second, increase and greater distinctness of the vascular striæ running from the base of the pyramids to the external surface. This latter species of injection, we are informed, often exists to a great extent without any corresponding injection of the rest of the kidney, and sometimes the red points composing the striæ are so much increased in size as to form considerable petechiæ, in which case the petechiæ usually extend to the surface, occupying the intervening spaces of the venous polygons(*a*). The true nature of these petechiæ has been the subject of much debate; but since

(*a*) These petechiæ were extremely common during the late scurvy epidemic. We remember a case in the Meath Hospital, which exhibited them to a remarkable extent. So intense was the disposition to hemorrhage in this case that the areolar tissue around the kidney was infiltrated with blood.

the investigations of Bowman, as minute as they were original, there seems to be no longer a doubt that extravasation into the tubuli uriniferi is the cause of this appearance. This seems agreed upon by all the writers whose papers we are analysing. The simple fact that the Malpighian bodies do not exist on the surface of the organ would be quite sufficient to disprove the idea of Rayer, and which is, we believe, the common idea, that vascular injection and hypertrophy of these bodies are the causes of the petechiæ. The size of these spots, and their irregularity, equally negative that notion; while the positive results of artificial injection of the tubuli in the hands of Mr. Bowman put it beyond doubt that these latter structures are the true locality of the spots seen on the surface. On the other hand, we may find the cortical substance blanched and anemic. There may be *no trace* of red colour, or there may be a mingling of a uniform light rose-colour with the characteristic pale yellowish tint of the renal substance, while the vascular points indicating the Malpighian bodies(*a*) are faintly visible. These conditions may co-exist with great vascularity of the surface and pyramidal bodies, where, as we have already observed, the veins are principally distributed. What are the causes of this blanched state of the kidney, on which Bright and all succeeding observers have laid so much stress? Here we find again the influence of general conditions of the system acting upon the vascular state of the kidney. Anemia is frequently the source of this appearance, as known to every one who has made even a few *post mortem* examinations. But the chief cause of this decolorization of the cortical portion of the organ is the occurrence of *abnormal deposit.* Rayer distinctly recognised that inflammatory diseases of the kidney are the most general causes of this decolorization, and has made a remark of great general pathological interest, as quoted by Dr. Gairdner, that both

"In the anemic and hyperemic conditions of the organ, the partial character of the vascularity is much more decidedly indicative of the presence of a morbid product, than its absolute amount."—p. 4.

We should, *à priori*, expect that deposit in the tubes would affect the Malpighian bodies, whatever may be their exact con-

(*a*) We think it useful for the information of those of our junior readers who are beginning the study of the kidney, to observe that the Malphigian bodies are visible with the naked eye on very thin sections, appearing as red points scattered over the surface of the section. A pocket lens renders them very distinct.

nexion with those structures, a question into which we shall not now enter. When we consider the firm character of the fibrous investment of the kidney, and the relations, whichever theory we may adopt, of the Malpighian glands to the tubules, we cannot come to any other conclusion than that sudden engorgement of the secreting portion must, even in health, be attended or followed by the diminution of its vascular supply. And Dr. Gairdner's inquiries have led him most absolutely to the opinion that this actually takes place, an opinion with which our own observations completely concur. Here he is so utterly at variance with the London pathologists that we must quote his own words:

"In such cases (namely, of decolorization of the cortical substance) the Malpighian coils of vessels, which, in a strictly normal specimen, may be observed filling the capsule, particularly towards its circumference, with red injection, are pale, bloodless, and compressed, sometimes maintaining their rounded form, at other times more or less angular."—p. 4.

As well as morbid deposit, Dr. Gairdner is of opinion that accumulation of their own secretions in the tubes can cause this condition. Here we have a question of great practical importance. Mr. Simon believes the products of inflammation have the effects upon the bloodvessels described by Dr. Gairdner; in this state, he informs us, numbers of the Malpighian bodies are extinct for all purposes of secretion; their vessels obliterated, their capsules wrinkled round them; they are dwindled, opaque, and bloodless. But Dr. Johnson, in reference to the most common of all the forms of exudation into the kidney, fat, is of opinion that the Malpighian bodies are in a state of congestion in this condition. Here we find, as we have just observed, Drs. Gairdner and Johnson at direct issue. Of course this is a matter to be investigated but in one way, by actual observation. We firmly believe Dr. Gairdner's doctrine to be correct, and the source of our belief is frequent and careful observation. Nevertheless, Dr. Johnson has given some *reasons* for his views, which may be profitably discussed, though by so doing we are rather anticipating the course of our analysis. It will be seen at once that this is a question of considerable moment, when we state that Dr. Johnson founds his explanation of the presence of albumen and blood corpuscles in the urine, upon the very circumstance *assumed*, that the presence of exudation in the tubes necessarily causes congestion of the Malpighian plexuses. Authors have long wanted something like a reasonable doctrine upon this subject. No wonder, therefore, that the views of Dr.

Johnson are already incorporated with the language of medicine(*a*). We must premise, and we say it with no offensive wish to cavil, that a suspicion naturally arises in our minds that Dr. Johnson's reasonings and *observations* were not a little biased by already known facts: the problem was, to explain the existence of albumen in the urine, in Bright's disease; but for this *fact*, it is certain that we should not have had the explanation, probably we should have equally wanted the observation upon which it is founded. We shall quote Dr. Johnson's own words on this point:

"The fat accumulates in the epithelial cells to such an extent as to produce engorgement and dilatation of the cells, and of the tubes which are lined by them; the consequence is compression of the capillary plexus surrounding the tubes, giving rise to congestion of the Malpighian plexus. This passive congestion of the Malpighian plexus leads to transudation of the serum of the blood, and sometimes to rupture of the delicate vessels of the plexus, and the consequent escape of the colouring matter and fibrin of the blood. These constituents of the blood pass into the tubes, and so become mixed with the urine. Their escape from the blood-vessels is the result of a mechanical impediment to the return of the blood consequent on compression of the veins by an accumulation of fat in the tubes"(*b*).

Did it never suggest itself to Dr. Johnson that the same exudation which compresses the capillary plexus surrounding the tubes may also compress the Malpighian glands? Nay, the afferent arteries(*c*) which feed those glands? Dr. Johnson knows quite well that the afferent artery and efferent vein penetrate the Malpighian capsule *close to each other*. Cannot a "pressure from without" act with equal force upon both vessels; and thus, instead of producing *congestion* of the Malpighian vascular system, cut off at one blow its entire vascular supply? For it must be remembered that there is no inosculation of the afferent arteries of the Malpighian bodies with each other. Each body has its feeding artery; cut off that supply and the condition is present which is alone necessary to effect its dwindling and atrophy. While, on the other hand, the "portal plexus" of the convoluted tubes offers one of the finest specimens in the body of vascular anastomosis; the vessels composing this plexus breaking up into innumerable ramifications immediately upon leaving the efferent vein, which, like the afferent artery, does

(*a*) Vide last edition of the classic works of Prout and Watson.
(*b*) Med. Chir. Tran., vol. xxix. p. 6.
(*c*) Vide article "Ren," in The Cyclopœdia of Anatomy.

not itself anastomose with adjoining vessels. We certainly can understand that pressure upon *this* plexus may be spent upon neighbouring plexuses, and, therefore, not be transmitted, or if so, very slightly in a retrograde direction,—namely, towards the Malpighian system of vessels. All pathological analogy favours this view. From Dr. Johnson's description we irresistibly get the idea that the Malpighian bodies lie all in one plain at the terminations of the tubes, like a row of Meibomian glands; and, that therefore, they cannot be affected by lateral pressure from abnormal exudation. But no one knows better than that gentleman the absurdity of this notion.

Dr. Johnson observes, that mechanical obstacles to the circulation through the heart or lungs may give rise to venous congestion, dropsy, and albumen in the urine; and that in two cases, to which he refers, there was congestion of the Malpighian bodies. Although we were unable to see the exact relation of this observation to his argument, we were certainly not fully prepared for what follows in his statement, that in neither case was there a trace of any organic disease of the kidney! Why this is the whole gist of the question: it was because there was no deposit in the tubes that the general venous obstruction reacted upon the Malpighian plexuses. If Dr. Johnson had examined the choroid membrane, or the vena salvatella, he would have found the same conditions. To come to a definite conclusion upon this point, we must examine a case presenting the combination of great obstruction at the central organ of circulation with marked exudation into the structures of the kidney. Here Dr. Gairdner comes to our aid. His sixth observation is a case of hypertrophy of the heart, with incompetency of the aortic valves, offering such obstruction to the circulation that the lungs were found "to be extensively studded with hemorrhagic extravasations." The kidneys offered the following appearances:

"The vascular striæ opposite the bases of the pyramids were distinct, the vascularity of the capillaries, and the Malpighian bodies not appreciable. The cortical substance was slightly and minutely mottled; the striæ of the pyramids, from their bases through two-thirds of their length, were masked by the presence of a white opaque deposit. The greater part of the deposit proved to be granules and globules of fat, which appeared to fill the tubes in every part of the organ; it was mostly unconnected with cells. The epithelium was very imperfectly developed, many of the nuclei being free."

In this case the urine was albuminous. But we have a direct statement of Dr. Gairdner's upon this question:

"A moderate or small amount of exudation, however, sometimes occurs in organs which present considerable vascular injection, and sometimes even a distinctly hyperemic condition. Such cases occur not unfrequently in the latter stages of heart disease. In only one or two cases have I found the vascularity much greater than usual. In these the kidneys were of large size, and contained very little exudation."—p. 38.

Dr. Gairdner indeed considers that the amount of exudation bears pretty nearly an inverse ratio to the congestion. In the very early stages of the former we find the kidney congested; but exactly as the morbid deposit increases so does the vascularity diminish. This last author has given a description of "partial distribution of the oleo-albuminous exudation"(*a*), which is greatly opposed to the views we are controverting. In this condition we observe small irregular patches of a pale colour, having an abrupt boundary by being marked off from the surrounding renal substance by a vascular ring. The pale patch, examined microscopically, presents the combined states of exudation to a great amount into the tubes, and depleted Malpighian bodies; while the vascular ring exhibits these latter in the highest state of congestion. This is an extremely interesting proof of the antagonism of *great exudation* and vascular congestion. We may remark, too, that Bright, Rayer, and all other observers agree in figuring and describing the granular kidney *as pale*. This is a great fact, which Dr. Johnson would do well to ponder over(*b*).

What then is the origin of the albumen in the urine in fatty kidney?—of the same element mingled with blood-corpuscles, epithelium, and fibrinous masses in the "desquamative nephritis" of these authors? Mr. Toynbee, and, we need scarcely add, Dr. Johnson, believe the presence of albumen in the urine depends exclusively on *obstruction* and congestion of the vessels of the organ; but the former observer considers congestion an antecedent to the other stages of the disease, and agrees with Dr. Bright, that "that condition of the blood-vessels does precede, and that necessarily, the deposition of fat, the enlargement of the organ itself or of its uriniferous tubes, or of any other of its vessels," a view supported by all pathological analogy. Dr. Johnson, in his first paper, found sufficient reason in the fat-

(*a*) *Plaques Blanches de Lymphe Plastique* of Rayer.

(*b*) We have not space to go further into this discussion, but we cannot help recommending an experiment to Dr. Johnson, which will be an *experimentum crucis* on the question. Let him perform Dr. G. Robinson's experiment of tying the renal vein in rabbits, after having exposed them for a certain period to the causes of scrofula, and so produced fatty deposits in the tubes of the kidney.

exudation for the presence of albumen ; and at that time he allowed the name of "Bright's disease" to *that state* alone, and which, dogmatically enough, he describes as being "primarily and essentially an exaggeration of the fatty matter which exists naturally in small quantities in the epithelial cells of the healthy organ;" and in his second paper he considers the anatomical elements of "Desquamative Nephritis" sufficient, by their obstruction to the circulation, to produce albumen in the urine. But we think our readers will find in the first of these papers sufficient evidence of a truer doctrine, namely, that albumen is a process of *secretion*, of abnormal nutrition on a mucous surface, and therefore patent to our observation in the urine.

"Exposure to wet and cold, and the consequent suspension of the cutaneous functions, may give rise to congestion of the kidneys, scanty, albuminous, and bloody urine, and dropsy."—p. 16.

Supposing the morbid action to attack the bronchia, we find no difficulty in understanding the *albuminous* products of a genuine inflammatory process *here;* and until pathologists give better reasons than Dr. Johnson has yet vouchsafed to us, we shall not believe the process in the one case to differ from the process in the other. But, says this gentleman, we have in the kidney a complex system of tubes offering, by their minuteness and number, obstruction to the passage of their own hyper-secretion, when plugged with fat or their own epithelium. Now having already a sufficient *cause* for a certain phenomenon, it appears an infringement of philosophical precepts to set up the existence of *another* cause for the same phenomenon. That we have sufficient *cause* for the presence of albumen in the urine, without assuming the existence of obstruction of the Malpighian plexuses, we have, perhaps, given already sufficient proof ; but we must again quote Dr. Johnson against himself :

"It is very generally believed that Bright's disease may have its origin in an attack of dropsy supervening upon scarlatina. It seems to me that this notion must be accepted with some modification. I have lately had an opportunity of examining the kidneys of three patients who died after an attack of scarlatina. In two of those cases there was albuminous urine, and in one there was dropsy; but in no case did the kidney present any of the characters of Bright's disease. In one there were no morbid appearances whatever ; in a second, there were unequivocal *products of inflammation ;* and, in the third, there was great congestion, with blood in the tubes, and other appearances which seemed to indicate an increased amount of functional activity."—p. 16.

It is unnecessary to enter into any analysis of this paragraph.

Sufficient that in one case there was no trace of organic disease; in another, unequivocal products of inflammation; in a third, "functional activity," which would, perhaps, have been termed something else by another pathologist; for, says Mr. Simon, "no interstitial effusion of lymph need exist in a gland to warrant us in accounting it inflamed; its inflammation may consist simply in functional derangement, and, during life, may be recognised only by admixing its albuminous products with those of normal secretion." We give that name to the case where a superfluous blastema becomes commingled with the secretion, as in active albuminuria, or undergoes interstitial development, as in cirrhosis of the liver; the term inflammation is applied with equal accuracy to both conditions. Dr. Gairdner appears to agree with Mr. Simon that the albumen in Bright's disease is, like sugar, a *secreted product;* but he has not offered a decided opinion on this point; though, having thrown aside Dr. Johnson's ideas, it is difficult to see to what other he can turn than that we have been defending. That the presence of oil in the cells of the kidney, even to an immense extent, does not necessarily entail the presence of albumen in the urine we have the testimony of Mr. Busk(*a*), who has observed undue secretion of oil by the kidney to a very great extent without the concurrence of albumen in the urine, as in certain cases of jaundice; and, contrariwise, albuminuria and all the phenomena of suppressed secretion of urine may exist, without any oil being discernible in the tubuli uriniferi. We have good reason for stating that Dr. Gairdner has made observations similar to those of Mr. Busk on the supposed connexion of these phenomena. Indeed we think it almost certain that the albumen of Bright's kidney bears precisely the same relation to the tube-casts of desquamative nephritis, as the ordinary products of catarrh bear to the false membranes of croup. The latter part of this analogy Mr. Simon has stated in direct terms. But we must leave the further discussion of this interesting question, and hasten to other parts of our subject.

Of the pathological elements of the kidney Dr. Gairdner considers first, as the commonest and simplest of all, *exudation*. He makes two sorts of division of exudation: first, as to its *locality*, whether within the tubes, within and around the Malpighian bodies, or in the inter-tubular tissue; secondly, as to its *nature*, crystalline (saline), oleo-albuminous, or purulent. It will perhaps be more convenient to discuss first, the less common and more unimportant of these divisions. Exudation is occasionally ob-

(*a*) Med. Chir. Trans. vol. xxix. p. 275.

served within the capsules of the Malpighian bodies, but this is by no means a common occurrence. Dr. Johnson constantly remarks upon the clearness and transparency of these bodies, while the tubes are filled with exudation; and while Dr. Gairdner describes its presence in this situation, of which he has given an excellent figure (fig. 9), he admits that it may occur within the tubes to a great extent, without at all involving the Malpighian bodies. When existing in large quantity, the tuft of vessels which normally fills the capsule, is completely compressed and shrunk, and in most cases invisible.

Dr. Gairdner speaks more doubtingly of exudation into the inter-tubular tissue: in the plate just referred to, this appearance will be seen figured. Mr. Simon disbelieves altogether that interstitial effusion effects much towards the final contraction of the kidney, which he thinks is consequent on a prior absorption of the intervening tissue. The meshes of the matrix come nearer together, and there is in a given space an excess of fibrous tissue, because the material, which originally expanded that matrix, is withdrawn.

We must premise a few observations upon the *crystalline* and *purulent* exudation before passing to the consideration of the most important locality of exudation, which will form the principal subject of our remarks.

One of the most original and instructive parts of the Essay before us is that treating of the "crystalline or saline matters deposited from the urine after secretion." Dr. Gairdner observes, that though frequently occurring as a sediment in the urine out of the body, urate of ammonia is no less frequently deposited from the urine contained within the tubes of the kidney. In most cases this is a post-mortem appearance, consequent upon the cooling of the body; occasionally, however, it is found in such quantity as to present to the naked eye the appearance of a distinct deposit. In these cases the cortical substance appears occupied by a white, or yellowish-white opaque deposit, very obvious also between the vascular striæ of the pyramids, particularly in the half nearest the cortical substance. This condition is consistent with a perfectly healthy state of the organ. The deposit is characterized by its ready solubility in dilute acids,—the acetic or nitric. Under the microscope it presents the appearance, when within the tubes, of a fine molecular shading, which entirely obscures the nuclei. That part of it which floats free is composed of molecules and granules, amorphous, angular, or approaching a circular form, but never accurately rounded. A great amount of this deposit may produce a distinct morbid condition of the kidney: in observa-

tion 2 we find a case of this kind. Both kidneys were tolerably firm in texture, the venous network of the surface was well injected, and the veins of the pyramids were in some parts full of blood; the cortical substance contained little blood; the vascular striæ and the Malpighian bodies were obscure. Here the tubes were completely filled with an opaque matter, which obscured the nuclei, and presented the appearances just described. The whole of this amorphous deposit was dissolved by adding a drop or two of dilute acetic or nitric acid. The Malpighian bodies were mostly destitute of blood. It is the more important to apply the tests mentioned to this kind of deposit, as it is frequently met with in connexion with fatty exudation, and, therefore, by their means we can judge of the relative proportions of these morbid elements. For further information upon this form of deposit, and more strictly crystalline deposits within the tubes, we must refer to the essay itself.

Pus-exudation will not long detain us, as, though a serious lesion, it is too uncommon to be a matter of much practical importance to the pathological inquirer. The most usual form, we are told by Dr. Gairdner, in which this exudation takes place, is that of small abscesses, rarely exceeding the size of a pea, irregularly disseminated through the cortical substance. The formation of abscesses having a distinct limiting membrane, or surrounded by condensed tissue, is, however, of extremely rare occurrence. Dr. Gairdner relates a case in which abscess of the kidney was found, together with purulent deposits in the lung. Probably in this case the abscesses had their origin from the general tendency to purulent deposit; but Dr. Johnson, under the head of acute suppurative nephritis, has given some details of a case which cannot admit of this explanation. An intemperate man was attacked with symptoms of suppurative nephritis, of which he died; the nature of the disease was detected, at the very commencement, by a microscopical examination of the urine, which revealed moulds from the tubes of the kidney, entangling pus corpuscles, and these latter free on the field. Numerous small points of suppuration were scattered through the kidneys, and the left contained two large recent abscesses.

We now come to consider the commoner forms of exudation into the tubes. Dr. Gairdner premises a useful observation on this part of his subject:

"The process of secretion in the kidney being, in fact, a normal process of exudation from the blood-vessels into the tubes, and one which, from its extreme complexity, is liable to very frequent derangement, it is not surprising that, of all the lesions of the

kidney, the presence of foreign matters in the tubes should be the most frequent. The greater part of such exudations, however, are either soluble in the urine, or readily carried away by it, and only come under the notice of the pathological inquirer in connexion with the alterations in that fluid. Of this kind are the albumen in Bright's disease, and the sugar in diabetes, besides a number of soluble substances which, although connected with morbid processes in the kidney or elsewhere, find a ready exit from the system, and do not remain to block up the tissue of the organ."—p. 11.

When, however, the secreting cells of the kidney receive substances which do not remain in solution, obstruction of the tubes takes place; the anatomical structure of the organ obviously increasing this tendency. The consistence, colour, volume, and weight of the kidney vary with the character and amount of exudation. Thus, the saline exudation just described generally renders the kidney soft, œdematous, and easily torn. When the exudation approaches nearly in colour to the kidney itself, we have great difficulty in distinguishing its presence, the organ presenting a uniform paleness, without any further apparent change.

The two great forms of exudation into the tubes of the kidney are the oleo-albuminous and the exudation of their own epithelium, combined with other inflammatory products. We shall take up the latter first. On this part of kidney pathology Dr. Gairdner is not full, and therefore not explicit. His views must be gathered from parts of his essay further advanced than that section upon which we are particularly engaged, especially part iii., on "Lesions of the Tubes and Epithelium." It is exactly upon this point that we have the fullest observations from Mr. Simon and Dr. Johnson, to whose admirable papers in vol. xxx. of the Transactions of the Medico-Chirurgical Society we very gladly refer our readers. We believe the following is a tolerable *resumè* of the facts given by thèse observers:

As glands secrete natural, so do they eliminate morbid, products. The mere *action* is the same in both cases; but in the performance of this latter duty these organs, owing to complexity of structure, and the narrowness of their canals, are very liable to embarrassment from changes in the physical qualities of their own secretion. It is needless to say, that the kidneys, while more often called upon than any other organs in the animal economy to execute these tasks, possess the greatest anatomical hinderances to their performance,—hence their frequent disease, disorganization, ruin. The poison, the *materies morbi*, may vary almost infinitely; but the effect, as far as the

kidney is concerned, is the same. As scarlatina presents such a poison, the elimination of which appears peculiarly to devolve upon the kidney, we are not surprised that "desquamative nephritis" is a very frequent concomitant or result of that disease. But we must introduce our readers formally to this new medical nomenclature. The name was *first* applied by Dr. Johnson to acute inflammation of the kidney. He has introduced it for the purpose of characterizing the chief phenomenon of the disease,—namely, desquamation of the epithelium lining the tubes. This does not look very philosophical on *principle :* but when we assure our readers that this great phrase only indicates what takes place equally in the bronchia and in the kidney tubes, they will perhaps concur with us that Dr. Johnson may as well have described a *new* affection of the urethra or air-cells, under the term of "*exudative inflammation.*"

Mr. Simon and this author ring varied changes on supposed acute, sub-acute, and chronic inflammation of the kidney; but as the lesions and symptoms do not essentially differ in these varieties, we shall not trouble our readers with their unprofitable discussion. Mr. Simon informs us that inflammation of the kidney depends on some humoral derangement of the entire system, and commences as functional excitement manifested in an act of over-secretion.

"The morbid material which thus stimulates the kidney in its struggle for elimination, will sometimes consist of products of faulty digestion, the lithates or the oxalates; sometimes of matters cast upon the kidney in consequence of suppressed function in other organs,—the skin or the liver; sometimes will be the mysterious ferment of a fever poison,—typhus or scarlatina. In these several cases, whatever variety may exist in the detail of their causation, the essential symptoms during life, and the essential anatomical changes, are strictly identical in kind, they vary only in degree. The *materies morbi* seeks to effect its discharge by means of an increased activity in the secreting functions of the kidney; it stimulates it; and the result of the stimulation is, not so much an increase of the watery secretion, as it is an augmented cell growth in the tubules of the gland. This acceleration of function is incompatible with maturity of the secreted products; the epithelial cells undergo various arrests of development, and become more or less palpably imbued with evidences of inflammation."—p. 144.

Fortunately, we have as satisfactory a mode of arriving at a knowledge of the existence of this lesion as we have of diseases of the chest by the stethoscope,—we mean, by the examination of the urine. In it we shall find albumen, epithelium cells in

all stages of disorganization, blood corpuscles, and fibrinous casts of the tubules enclosing all those forms, and probably oxalic acid or lithic acid crystals, and *rarely* pus corpuscles. The whole range of medicine does not contain more satisfactory diagnostic marks of diseased action than do these appearances in the urine afford of the particular lesion in question. The cells of epithelium may be quite healthy, or they may vary almost infinitely, presenting, according to Mr. Simon, "a variety of indefinite shapes, constituting a series of connecting forms between the pus globule and the healthy gland cell." The fibrinous casts are very characteristic products of the morbid condition of the tubes. These are exceedingly delicate, transparent, and colourless cylinders, containing frequently in their mass some of the cell-forms just mentioned, with blood corpuscles. We have also seen lately these masses having oil globules attached to their surface. The case is doubtless one of those described by Dr. Johnson as "a combination of fatty degeneration, with desquamative nephrites," an idea supported by the history,—that of a drunkard,—taken in connexion with present symptoms, pain in the back, intense anasarca, strongly albuminous urine, containing debris of epithelium, and those peculiar fibrinous masses, all coming on within a few days after marked exposure(*a*). When we observe that these appearances in the urine often exist before the most delicate tests of albumen reveal its presence, and so warn us of the coming evil, and that they are still to be seen in that excretion after albumen has ceased to be secreted, in those periods of quiescence so likely to give a false security to both practitioner and patient, and thus keep us on the alert lest disorganization should creep on while we are in ignorance,—we need no longer insist that a microscopical scrutiny of the urine is as necessary to the full enlightenment of the physician, as a stethoscopic examination is already and by every one allowed to be in the diseases of the respiratory and circulatory organs. In both cases the mere symptoms are equally liable to deceive the most able, the most experienced.

We have already hinted that this desquamative nephritis is constitutional in its origin; that this action, so disorganizing in its results, is itself but an increased activity dependent on some more general condition. Dr. Johnson has stated his opinion to be that the predisposing cause of the disease is the pre-

(*a*) It is impossible to over-estimate the advantages derived from an accurate examination of the urine in this case. It was the most clearly marked case of idiopathic nephritis in the adult we have ever seen; and the remedies such a diagnosis would suggest were attended with great success.

sence in the system of abnormal and irritating products, the results of mal-assimilation; and it is in the process of their excretion that the kidney suffers. It is highly probable that his opinion is correct, that mere exposure to wet and cold would never produce that form of disease under consideration, if the person so exposed had been previously healthy and well nourished. It is the *quality* of the blood sent to the kidney, and not the *quantity* of this material, which produces the disease. This is an important observation with reference to the general pathology of the affection, but we must not forget that the kidney is itself the seat of a morbid process, which, unless timely checked, will end inevitably in utter ruin of the organ.

The morbid anatomy of desquamative nephritis still requires to be worked out by careful observers. We cannot be said to know much of a state which has been made the subject of such very contradictory descriptions. As far as the naked eye is concerned, we may state that, in the *first stage*, the organ retains pretty nearly its normal dimensions, and that its vascularity is increased. In the more advanced stage it becomes firm, contracted, and granular, while its vascularity is *undoubtedly* diminished. In reference to this latter stage Dr. Gairdner observes, that while those changes are proceeding, the capillary vessels, which have ceased to be subservient to secretion, are usually obliterated. In consequence of the double obliteration of vessels and tubes, there is a considerable degree of atrophy in the diseased parts; and as the atrophy takes place, at first, chiefly in the cortical substance, great irregularities of the surface generally supervene. Thence arises the appearance, so well described and figured by Dr. Bright (plate iii. fig. 2), in which, from the atrophy of the cortical substance, the bases of the pyramids "are drawn towards the surface of the kidney." —p. 46. The atrophied portions of the kidney are usually exsanguine, hard, and tough. Dr. Gairdner does not appear to agree with Dr. Johnson, that a kidney in this condition ever exhibits the appearance of the granulations of Bright, which, it must be remembered, are owing to exudation *within the tubes*. According to the former observer, these granulations exist only when oleo-albuminous exudation supervenes on desquamation, or *vice versâ;* and that the atrophy proceeding around the diseased tubes gives rise to the irregularities and extremely tuberculated surface of the latter stages of Bright's kidney. We are disposed to agree with Dr. Johnson that the inflamed kidney does become granular; that the tubes and the vessels around them become atrophied, indeed almost annihilated, is unquestionable, and all the result of excessive epi-

thelium-exudation *within the tubes.* We have only to consider the existence of these conditions confined to certain portions of the kidney, and we have all that is necessary to form a granulated surface; but these granulations would not be the "granulations of Bright," according to the definition that they result *always* from exudation within the tubes, for it is obvious that in desquamative nephritis the granulations will be produced by the prominence of the healthy portions of the organ rendered salient on the surface by the atrophy of the diseased tubes(*a*).

The microscopic analysis of nephritis is much more satisfactory. In the first, or acute stage, its main characteristic is the engorgement of the tubules with an "uneliminable excess of crude and vitiated secretion." As well as with epithelium very slightly altered from its normal character, we observe the tubes to be filled with blood, amorphous matter, and possibly lithic acid, or oxalate of lime crystals. We repeat, the characteristic of this condition is an excessive development, but slightly altered, of the epithelium which lines the urinary tubules, by the accumulation of which these latter are rendered opaque. These appearances are confined to the cortical portion of the organ. The pyramids, it is stated, sometimes contain an increased number of cells, owing, probably, to their lodgment in the passage from the convoluted to the straight tubes. We are told by Dr. Johnson that, in the majority of cases,

"Under judicious treatment, the noxious matters are effectually eliminated; the vascular congestion and the desquamation of the urinary tubules simultaneously diminish; the cells which were thrown into the tubes are gradually washed out, and the kidney is completely restored to its original healthy condition"(*b*).

But this happy result does not always occur. The diseased process continues and the tubes become more and more plugged with epithelium ; but it is obvious that there is a limit to this process. Though some of the cells may escape with the urine, what remain become so closely packed that their further formation is impossible; and, consequently, the processes of cell-development and secretion within tubes so plugged are arrested. Like all other abnormal exudations, the cells undergo, in course of time, disintegration. The disintegrated cells, attached to fibrinous moulds of the tubules, *results of the same process*, are washed out by the current of liquid flowing through the

(*a*) There appears good reason to think that the highest amount of granulation arises from the combined conditions mentioned by Dr. Gairdner, though Dr. Johnson is at issue with him on this point.

(*b*) Med. Chir. Trans., vol. xxx. p. 170.

conduits, and thus become objects of microscopic research in the urine. Dr. Johnson has made the important observation, that when, the tube becoming filled with its accumulated contents, the processes of cell-development and of secretion have once been arrested, it never recovers its lining of normal epithelial cells. We are now arriving at the advanced stages of this affection, when we see the basement membrane entirely denuded of epithelium, or containing a few granular particles of the old decayed epithelium, or, rarely, seen to be lined by "small, delicate, transparent cells." Upon this condition of the tubes supervene their gradual wasting, and a generally diminished supply of blood. We are not surprised that these conditions are revealed to the naked eye by the firmness, contraction, and granulation of the organ. However different the opinions of the various authors we are reviewing, as to the condition of the Malpighian bodies in *other* morbid states, in the particular one under consideration it is universally allowed that they also partake of the wasting occurring in the other portions of the kidney. The atrophied portions exhibit an abundance of fibrous tissue, which is but little more than the atrophied and altered remains of the basement membrane of the tubes, "whose cavities are obliterated, and thus what were tubes assume the appearance of transparent ribands, dotted here and there with small oval nuclei, which, when seen at the edges, appear to be enclosed between the layers of membrane"(*a*). With the obliteration of the tubes, obliteration of the capillaries, rendered of course unnecessary by the cessation of the secretion they were destined to supply, proceeds *pari passu*. But the tubes sometimes undergo a very different change. Deprived of their epithelium, they yet appear to have the power of secreting water; and, according to Drs. Johnson and Gairdner, they by their dilatation constitute the *cysts* so frequently seen in the kidneys. The former observer bases his opinion that these cysts are only dilated tubes whose lower end has been in some manner *closed*, on the following grounds:—that the tubes are often seen much dilated and thickened; that as the inner surface of the tubes has the appearance of being endowed with the power of secreting water, so the cysts usually contain a simple serous fluid; that as an accumulation of oil occasionally occurs in the tubes, so the cysts are in some instances filled with the same material; that there is no reason to suppose that these cysts have any other origin. Dr. Gairdner, too, believes, from the

(*a*) Dr. Gairdner's Essay, p. 44. Dr. Gairdner has confounded Mr. Simon's opinions, as to the fibrous tissue of the kidney in chronic nephritis, with those of Dr. Johnson, who really agrees with himself.

occasional appearances of alternate distension and constriction presented by the tubes, that cysts may be formed by the occlusion and isolation of portions of tubes; though he admits he has not been able to obtain demonstrative evidence on this point. Mr. Simon gives a totally different opinion concerning the nature of these bodies, and has devoted a very considerable portion of his paper to the discussion of their origin. He believes that they are the result of the gland-germs effused from the obstructed tubes by the rupture of the "limitary membrane;" that they are essentially of a *reparative nature*,—"the last phenomena of the original disease, and the first of the attempted compensation." He states they are liable to arise equally in the *fatty* as in the inflamed kidney; and that they have a *function* to perform, being

"Organized for secretion into their own cavities, so as at least to withdraw from the blood, if they cannot eliminate from the body, the material which fills them."—p. 153.

It strikes us that their *function*, as laid down by Mr. Simon, even supposing both kidneys to have "completely undergone a vesicular transformation," can be of very small importance in the animal economy. We are told, too, that it is the withdrawal of these cysts which causes the contraction of the organ, and that in the uncontracted specimen a false appearance of size is maintained by the presence of the adventitious cyst growth, which forms a "succulent surplus" over and above the real kidney structure. All the cysts found in the kidney, from the size of the normal gland cells to that of a walnut, have, according to Mr. Simon, the same origin. Dr. Johnson so utterly discredits these opinions of his fellow-worker as to hint the probability of his having mistaken the normal disposition of the tubuli for a cystic structure; while Dr. Gairdner doubts if the tubes in diseased kidneys ever suffer rupture from over-distension; and is quite satisfied that the vesicles sometimes seen in the kidney are exceptional productions, and not invariably connected with the progress of desquamative degeneration, for proof of which he has referred to his own observations in four cases. It cannot be denied that we may search kidneys in every stage of desquamative disease without finding these bodies, while it is quite well known that cysts constantly occur in kidneys possessing no other abnormal appearance. We think it very probable that the origin of these bodies is by no means the same in all cases. While in the main we agree with Dr. Johnson's views on cyst-formation in the kidney, we are not so certain as that gentleman appears to be, that the Malpighian bodies have no connexion

with those structures. Dr. Gairdner has recorded one instance in which the Malpighian capsules were occupied by distinct cysts, and we see Dr. Lever has recently exhibited a monstrous fœtus at one of the medical Societies of London, where the Malpighian bodies had undergone a cystic transformation. This very interesting subject requires further observations before satisfactory conclusions can be arrived at.

We have now to make some brief observations concerning *oleo-albuminous* exudation into the secreting cells of the kidney. This subject has derived great illustration from the labours of Dr. Johnson, who has employed the term *fatty exudation* in a very restricted sense, meaning by it the exudation of fatty molecules and granules into the secreting cells of the tubules, and as having no connexion with the process of inflammation. We have preferred the term proposed by Dr. Gairdner, as indicating the *nature* of the deposit, and its frequent connexion with the true products of inflammation. On the question, whether fat in the kidney be always the result of a totally different condition from inflammation, as assumed by Dr. Johnson, we most fully agree with Dr. Gairdner, that the terms "inflammatory" and "non-inflammatory" are more applicable to the modes of invasion of disease of the kidney, than to differences in their pathological anatomy, and that *none* of the lesions of that organ can be correctly considered as having an exclusive connexion with *any* specific pathological cause. We would particularly recommend the section of Dr. Gairdner's essay on "oleo-albuminous exudation from the blood-plasma" to the careful perusal of our readers. The association of fat with non-inflammatory conditions of the system is almost universal in this country. It would be hard to conceive a greater pathological mistake, or one whose tendency is more injurious to practice. Fatty kidney, heart, liver, steatoma of the arteries, are all put in the same category, the general expression of which may be said to be *struma*, the general treatment *tonic*. But it is quite certain that all inflammatory products are more or less fatty; that, for instance, the granules of the exudation-corpuscle of pneumonia are almost entirely composed of *fat*. We are informed, indeed, by Dr. Gairdner (who quotes the researches of Guillot), "that in some of the forms of so-called pneumonia, the quantity and size of the fat globules and granules are such as to constitute a true fatty degeneration of the lung, in the same sense in which the term has hitherto been more familiarly applied to the corresponding lesions of the liver and kidney."—p. 20. We make, therefore, *in limine*, this objection to Dr. Johnson's paper, that, though in it the morbid anatomy of fatty kidney is

accurately and beautifully described, it contains the weighty error of associating this condition with an exclusive pathological cause, always different from, nay, *antagonistic to* inflammation. This, if an error, is of course a *practical* one, and one of obvious interest in the question of treatment; yet in controverting the doctrines of Dr. Johnson on this point, we desire to be understood not to disbelieve that fatty deposits in the secreting cells of the kidney are often the product of general states of the system, to which the term inflammation can by no means be applied. No one can doubt that the fatty liver and kidney of phthisis do not owe their fatty degeneration to inflammation of those organs. Similarly, we believe, Dr. Johnson has not erred in ascribing the particular appearances in the kidney, which form the subject of his first paper, to general constitutional conditions *in a great number of cases*. Our objection is only to the *exclusiveness* of his doctrine. Of course Dr. Johnson asserts that congestion is never an antecedent to the fatty degeneration, an opinion which is contradicted by all other observers, including Dr. Gairdner. But as we have already touched upon this subject, we shall only state here that he explains the undeniable presence of congestion in these cases, by assuming it to result from pressure on the portal plexus, reacting upon the Malpighian bodies. *Active congestion* he ascribes to the increased action of one part of the kidney, owing to the other portions of the organ being put *hors de combat* from morbid deposit.

Dr. Johnson has made the important remark, that fatty degeneration of the kidney is very frequently attended with the same conditions in the liver, valves of the heart, and the arteries. In twenty-two cases of Bright's disease, there was in seventeen marked fatty degeneration of the liver; in four out of the remaining five there was a decided increase of fat in the hepatic cells(*a*). Dr. Johnson has also given fresh proof of its frequent association with tubercular disease of the lung. In forty-nine cases, this latter disease existed in fourteen. From these and other circumstances, Dr. Johnson appears fully entitled to draw the inference, that the source of the disease must be looked for in the processes of digestion and assimilation. The processes of primary or secondary assimilation, or both,

(*a*) The existence of fat in the hepatic cells is well known to be the natural condition of these cells. Dr. Johnson considers some of the cells of the kidney to contain a small amount of fatty granules as a normal constituent, an opinion with which Dr. Gairdner does not agree. We believe the former observer to be right. If a small amount of fat be not natural to the kidney, we have yet to see one in a healthy condition.

fail in regard to this fatty matter, which, not undergoing the changes requisite for its ready elimination from the system, is thrown into the circulation. An effort is made to carry it off by the liver and kidneys, whose secreting cells, unable to excrete it with sufficient freedom, thus become charged with the morbid material. The naturally small amount of oil in the urine, not suffering any notable increase in Bright's disease, this effort of the kidney-cells must, in great measure, be considered as unsuccessful. Hence the obstruction in the tubes of the organ, and a serious interference with its functions. All this Dr. Johnson describes well, as the product of an attempt on the part of the kidney to eliminate a mal-assimilated element. But does not this bring the true "morbus Brightii" of our author into the same category with "desquamative nephritis"? In both, the kidney is not primarily at fault. In eliminating the *materies morbi*, in both cases, let it be remembered, a *general condition*, the organ suffers. True in the one case the "materies morbi" is itself part and parcel of the disease, and itself obstructs the secreting structure in the process of elimination, while in the other it obstructs only by inducing a vast increase in the number of epithelial cells. True the one is the result of conditions, for the most part permanent and incurable; the other of states, temporary, and to a great degree under our control. But the analogy in the ultimate principles on which these affections rest is striking and important. The compound granular corpuscles, so generally attendant on the inflammatory process, are present in the latter of these conditions, and almost insensibly shade off into the large fat-globules of the former state. May not also, as Dr. Gairdner has suggested, this so-called fatty degeneration, considered so exclusively *chronic*, be in reality but the advanced stage of processes more or less acute, which have not been fatal in the first instance? And we are told by Gairdner that *desquamation* is more often associated with the oleo-albuminous than with any other state.

Dr. Johnson believes the cardiac disease, so often associated with Bright's kidney, to be the result of a common condition of the blood in the majority of cases; at the same time giving his assent to the common doctrine, that the cardiac disease originates in some cases in the "morbid and irritating condition of the blood, consequent on the impaired function of the kidney."

In Bright's disease the urine occasionally contains a great increase of fat; but this is not a very common occurrence. The fibrinous casts we have already mentioned, as occurring in des-

quamative nephritis, are also observed in the fatty degeneration; but they are rendered characteristic of this affection by entangling oil globules and cells containing variable quantities of fat; epithelial cells, however, are frequently seen in the urine when the fibrinous casts are absent, and form the most important diagnostic mark of the special renal lesion. In some experiments made by Mr. Simon upon cats which had been confined in a cellar, a considerable quantity of free oil globules, as well as epithelial cells enclosing various quantities of oil, were found in the urine previously to the appearance of *albumen* in that excretion. As the fat in the urine diminished, it became turbid, from coagulated albumen, on the application of heat and nitric acid. The kidneys were found in an advanced stage of Bright's disease. These observations clearly prove the great usefulness of a microscopic investigation, as well as a chemical one, when there is any doubt as to the true nature of the case.

The characters of the fatty degeneration, when slight in amount, are by no means very obvious to the naked eye. Nothing is more common than to mistake a fatty kidney for some other state, and *vice versâ*, of which Dr. Gairdner has given some proofs. But, as the accumulation increases, the kidney becomes granulated and mottled on the surface; and, indeed, there is generally slight mottling of the whole cortical substance:

"The smooth, mottled kidneys are such as have the greater number of the tubes in the cortical portion almost uniformly gorged; the gland is often much increased in size by the great amount of fat in the tubes; the vessels are much compressed; and the surface of the kidney sometimes presents an almost uniform yellowish white colour, with here and there a few vessels which have escaped obliteration. These are generally cases which have run a comparatively rapid course. The secreting function of the kidney has become greatly impaired, and death has been the consequence. The kidney which has arrived at this degree of fatty engorgement probably never becomes atrophied"(*a*).

In general terms, it may be stated that kidneys suffering under a great amount of fatty exudation, are somewhat larger than natural (though, according to Dr. Inman's observations, their *specific gravity* is diminished); their consistence lessened; their colour of a pale, and almost uniform yellow, both on the surface and throughout the whole cortical substance, which intrudes very much upon the pyramids; and they are, for the most part, smooth, or with a slight tendency to

(*a*) Med. Chir. Trans. vol. xxix. p. 9.

granulation. When the accumulation of fat takes place less rapidly and uniformly, some of the tubes becoming gorged with fat form prominent granulations; and these by compression of surrounding parts (and so producing their atrophy) become salient on the surface, forming the well-known "granulations of Bright." But it is probable, as we have already observed, that Dr. Gairdner is correct in ascribing great contraction and irregularity of the organ to *exudation* and *desquamation* combined.

On examining a section of the cortical portion with a power of 100 diameters, sets of convoluted tubes are seen crowded with their fatty contents. On examining it with a power of 350 or 400 diameters, the fatty material is seen to be contained within the *epithelial cells.* The recognition and distinct statement of this fact by Dr. Johnson has probably enlightened us more on the true pathology of the kidney than any other that can be named. It almost never happens that the whole of the tubes and cells are equally the seats of fatty deposit. The presence of oil globules in a free state is accounted for by Dr. Johnson, by the *rupture* of the cells in which they were contained. Dr. Gairdner believes that they are sometimes seen interspersed among the nuclei and cells as their original condition. This latter author observes that "fatty deposits may exist in the cells of the kidney in large or small granules. It sometimes takes the form of a nearly molecular deposit; in this case it forms a mere shading, obscuring the nucleus, and rendering the cell more or less opaque. At other times cells may be seen in different states of fulness, their contents being granules of nearly equal size, and not larger than from $\frac{1}{3000}$ to $\frac{1}{300}$ of a millimetre. This is the granular corpuscle or inflammation-globule of German writers. Again, the contained granules may be very unequal in size, the cell being irregularly distended or partially filled with granules and globules, from $\frac{1}{3000}$ up to $\frac{1}{100}$ of a millimetre."

In the *treatment* of this affection Dr. Johnson insists with great propriety upon the importance of invigorating the general constitution by hygienic means, and by the administration of chalybeates We have given sufficient reasons for the application of local remedies also, at least in its early stages. Dr. Johnson, somewhat naively, considering the pathological theory he upholds,—observes that the kidneys will require some special treatment, with a view to relieve *congestion,* which necessarily interferes more or less with the function of the gland, if it do not increase the tendency to fatty accumulation. Local bleeding may sometimes be called for, and it is a measure often followed

by great relief, and a manifest improvement in the secreting power of the kidney.

Under the head of "Lesions, chiefly affecting the Vascular System," Dr. Gairdner has described, by the name of "waxy degeneration," a morbid condition of great interest. The ultimate elements of this affection are obliteration of the capillaries, and dilatation, with thickening of the tubes. He divides the disease *into two stages*. The first stage is attended with great irregularity of distribution of the superficial veins, hence the characteristic "marbled" appearance in it. On section, the cortical substance has considerable volume, and presents a smooth, glistening, almost semi-transparent appearance, which, according to him, cannot be better distinguished than by the term "waxy." It has a general pale, flesh tint, while the bases of the pyramids are indistinctly marked. The greater portion of the cortical substance is impervious to injection. In the second stage of the waxy kidney, the organ is perfectly pale throughout, with the exception of a very few stellated superficial veins. It is still heavy and voluminous, acquires additional firmness and elasticity, and has the general appearance of a true non-vascular texture. It is of a light fawn-colour, which extends to the pyramids, the bases of which become still more confused and intermingled with the cortical substance than in the marbled kidney. The capsule is more firmly adherent to the external surface than in health. Dr. Gairdner remarks that, from the pale and yellow appearance of the kidney in this stage, it is very apt to be mistaken for an extreme degree of the fatty degeneration, and alludes to the *lustrous character* of the cut surface as a valuable diagnostic between these two states. There is not unfrequently an entire absence of exudation in these cases. The capillary vessels surrounding the tubes are not visible, and in their place there is fibrous tissue, while the tubes undergo dilatation and thickening, and become twisted and varicose. Bright and Rayer have figured this condition, though unaware of its real nature, and have described it as connected with albuminous urine and dropsy, with which opinion Dr. Gairdner entirely concurs. From various considerations he considers *intense congestion* the starting point of this peculiar lesion.

We intended to present our readers with the general conclusions at which Dr. Gairdner has arrived concerning the pathology of the kidney, but the already formidable length of this article compels us to waive our intention. We cannot conclude without earnestly recommending his essay to all who really wish to know the minute pathology of the kidney.

Those who consult it will find the various lesions most accurately and copiously figured, and illustrated by excellent cases.

Clinical Lectures delivered in the Theatre of Mercer's Hospital during the Session of 1847–8. By JAMES F. DUNCAN, M. D., &c., Assistant Physician to the Hospital. Dublin, M'Glashan. 1849. 8vo. pp. 122.

THE stimulus given to the acquirement of a practical knowledge of medicine, as obtained at the bed-side of the patient, by the labours, in the first instance, of Whitley Stokes and of Graves, seems to have produced most excellent results in the method of clinical instruction so generally followed at present, not only in the hospitals of our own city, but all over the British empire. The student is now taught to inquire and investigate for himself; his observations directed in the proper channel, his errors explained and corrected, he is no longer compelled to adopt, without due examination on his own part, the *dictum* of his teacher; nor allowed, as in former days, to grope along in darkness, acquiring a sort of empirical knowledge of his profession. This plan has its good effects, both on the teacher and the taught: the former is stimulated to a careful discharge of his duties; he shares in the emulation of his pupils for the attainment of a perfect knowledge of every case that may come under his care; and he is careful to keep himself acquainted with every new fact and every new discovery in medicine. Its effects on the latter are best exemplified by the recent valuable additions to pathology and therapeutics, so many of which are due to the junior members of the profession.

These considerations have been suggested to us by the perusal of Dr. Duncan's clinical lectures, which, though not remarkable for much originality of observation or new discovery, are characterized by judicious investigation and patient inquiry, and evince the author's acquaintance with the newest doctrines of pathology. Intended for students, to whom they "were originally delivered, *vivâ voce*, from rough notes prepared for the occasion, without having been written out in a form suitable for publication," the style of the lectures could not be expected to be as ornate as if they were carefully prepared for the press, yet it is smooth, very readable, and with but few redundancies.

The volume contains only ten lectures, which appear to have

been delivered simply as illustrative of cases under treatment in the hospital, and not with the view of describing any special class of diseases. In the first lecture the author describes shortly the various causes on which diarrhœa is supposed to depend, and, glancing at the treatment adapted for some of the forms, bears his testimony to the efficacy of strychnia(*a*) in those cases attended with absence or deficiency of bile in the evacuations, for which it was first recommended by Dr. Bardsley of Manchester. Dr. Duncan believes that the efficiency of this remedy depends on its action in promoting secretion: he does not, however, state in what doses he prescribed it, or with what other remedies he combined it.

The fourth lecture is devoted to the consideration of a case of inflammation of the membranes of the spinal marrow, occurring in a young girl aged 18, and which readily yielded to the ordinary antiphlogistic treatment,—general and local bleeding, calomel and James's Powder, and tartar emetic. The author takes the opportunity afforded to him by it, of giving a short account of the epidemic cerebro-spinal arachnitis, which was so well described in the second volume of our New Series by Dr. Mayne. He considers his case to bear remarkable *similarity* to the detail of symptoms given by that physician; but we must confess that we think Dr. Duncan would have rendered his observations more practically useful had he directed attention to the *dissimilarity* which the symptoms presented, as a means of diagnosis between the two affections. For example, the great collapse, the muscular rigidity, the tetanic expression of the countenance, the twitchings of the muscles, and, finally, the convulsions, all of which were absent in our author's case of spinal arachnitis; and, further, the marked difference in their amenability to treatment. A singular typographical error occurs in this case: speaking of the specific gravity of the urine, it is stated that the *thermometer* showed it to be 1035.

The fifth lecture is taken up with an account of Bright's disease of the kidney, and a short but good *resumè* of Dr. Johnson's researches on its pathology. That these are not in every respect in accordance with the most modern investigations, our readers will learn from the lengthened notice of Dr. Gairdner's labours in the preceding review, which, however, were not pub-

(*a*) Dr. Duncan spells this word strychnine; in doing so he follows a very common error of medical writers and prescribers. Alkaloids should be written with the termination *a*, as *strychnia*, *quina*, *morphia*, *narcotina*, &c.; vegetable acids with the termination *ic*, as *tartaric*, *citric*, *meconic*, *tannic*, *gallic* acids, &c.; and neutral vegetable principles with the termination *in* or *ine*, as *salicine*, *cantharidine*, *piperin*, *picrotoxine*, &c.

lished when Dr. Duncan delivered this lecture, although his volume bears the date of the present year.

The greater number of the remaining lectures are devoted to the consideration of several cases—some of them rare examples—of diseases of the lungs and heart. In these the junior practitioner will find many useful hints on diagnosis, which prove that the author is a pains-taking physician, and a careful investigator into the varied phases of disease.

We have only one further remark to make before concluding, and that regards a habit which is much too general amongst medical writers, of contracting the words in their prescriptions to as few letters as possible, scarcely ever giving a termination. If our own language, with which it must be supposed the profession is more familiar, were to suffer the same mutilations, few amongst us could decipher an author's meaning; how much less, then, can such Latin be read or understood? Medical Latin is, in all conscience, barbarous and ridiculous enough without being rendered more absurd by such wholesale amputations as it is often compelled to undergo. We cite at random some of Dr. Duncan's directions in his prescriptions; but, in doing so, we beg that he will understand our observations do not by any means apply to him alone; he has only followed bad example. The following are given *verbatim:*

"Ft. pil. 3tiis horis sing. sum.——Infricet. ung. hydr.——Mist. Pector. c. soda.——Vini ʒiv.; beef tea; cont. cæt."

The Editor of this Journal has, we think, very properly refused the admission of long prescriptions into its pages; the substance of the prescription, or the dose of the chief ingredient, being merely given in every instance.

Reflections on Organization, or Suggestions for the Construction of an Organic Atomic Theory. By HENRY FREKE, M. B., M. R. I. A., &c. Dublin, M'Glashan. 8vo. 1848. pp. 80.

WE believe, notwithstanding what some electricians say to the contrary, all matter to be essentially inert; we believe it to be incapable of originating, directing, or ending its own motions; and we think those physicians argue most logically who regard both motion and rest as the result of forces acting upon, always accompanying, but not the less abstractedly distinct from the substances upon which they operate. We are willing to admit that the forces capable of producing material motions can only manifest themselves when matter is congregated in masses.

Gravity, cohesion, repulsion, can only be called into operation between masses or particles of matter; and if a single particle can be imagined existing alone in space, we do not see how the forces we have enumerated could be at liberty to act. Under these circumstances, Dr. Freke, the author of the work before us, considers that the forces alluded to would be latent, and that the presence of other particles or masses, calling these forces into exercise, entitles us to consider them as reciprocals, or stimuli of those causes of motion. It is admitted that, in a state of rest, the electrical forces with which all material bodies are endowed remain in a condition of latency; and it is as a consequence of molecular motion, whether produced by friction, change of state, chemical action, or any similar cause, that these forces are called into activity. It is known that the electrical forces, thus becoming manifested, display distinct properties; and, if we understand Dr. Freke aright, he regards the one as the reciprocal or stimulus to the other. Perhaps, however,—for he is not very clear on this head,—he may consider the molecular motion, which renders electricity manifest, its true reciprocal or stimulus. So far we find no occasion to dispute Dr. Freke's postulates; but there is another position which he assumes, which we cannot say that we disagree with, for we are not sure that we understand it: it is to the effect that, when the forces are aroused into activity by matters reciprocal or stimulus, "they then radiate centrally, giving manifestation to active results, and range the reciprocal or stimulus round the inanimate matter, in the form of an envelope surrounding a nucleus." We have already confessed that we do not clearly understand what is meant in this postulate. A current of cool air, passing over the surface of a pool of water, may cause the upper stratum to be converted into ice; latent heat becomes sensible, and the current of cold air must be considered as the reciprocal which called it into manifestation. Does the cold air range itself round the ice as an envelope surrounds its nucleus?

A piece of silk is rubbed to a glass rod, and the latter becomes electrified. Surely the silk does not usually constitute an envelope for the glass rod. It may be, however, that we do not comprehend Dr. Freke's meaning; and we shall, therefore, proceed to develope in as few words as possible his application of these principles to a theory of organization.

We cannot conceive life without motion, and inasmuch as all motion is produced by the operation of force upon inert matter, organization must be looked upon as the resultant of certain forces. Let us call the force by the operation of which

organized beings are produced, plastic force, vital force, organizing agency, &c., or any other name we will, the principal objects which we must maintain in view, forgetting a definite idea of its nature, are the particulars in which it differs from the other recognised forces, and the peculiarity of the motions which it produces in matter. Now, one of the most striking characteristics of this force, as displayed in the process of organization, is the occurrence of attraction between organic molecules of different kinds, and of repulsion between those which differ from each other. In these respects it differs from all other known forces; for although cohesion may unite particles of a similar nature, it will equally draw together those that are dissimilar; and affinity, as well as electricity, joins different kinds of matter, while they cause repulsion to occur between substances of a similar nature. But while the vital force, in its rule over nutrition, controls matter in the manner we have described, there are other phenomena which occur in organized beings, which are discrepant with these attributes. The organic molecules which are attracted like to like in the building up of the tissues, are themselves composed of elements very different in their several natures,—oxygen, hydrogen, nitrogen, carbon. The elements which enter into most organic molecules differ extremely from each other in their properties; and in attributing their formation to the action of the vital force, it has always been considered a difficulty how the same force can, in nutrition, produce attraction between *similar* compound molecules, and in the construction of those compound molecules cause *dissimilar* elements to unite. This Gordian knot Dr. Freke has cut with singular facility. According to him the organic molecules were ordained at the time of the creation, and they have since had nothing to do but unite with each other according as necessity required.

As for the rest, the path of explanation is easy. Organizing energy remains latent in these organic molecules, until called into activity by the presence of a reciprocal or stimulus, when it radiates centrifugally, and ranges the reciprocal round it as a cell of which the organic molecule constitutes the nucleus. But in becoming manifest the organizing energy diffuses itself, and the organic molecule, becoming deprived of it, falls under the influence of chemical forces, and thus, to use the expressive words of Dr. Freke, "death is essentially a part of life."

We regret that we cannot devote more space to this interesting work. In this age of paste and scissors book-making and compilation it is pleasant to meet with an author who has the originality to think, and the boldness to express his medi-

tations. We hope to meet with Dr. Freke again, when he shall have more completely investigated the mine which he has opened in the little volume before us.

Elements of Chemistry, Practical and Experimental, including the most recent Discoveries and Applications of the Science to Medicine and Pharmacy, to Agriculture, and to Manufactures. By Sir Robert Kane, M. D., M. R. I. A. With 236 Illustrations. Dublin, Hodges and Smith. Second Edition. 8vo. pp. 1072.

The science of chemistry has made an extraordinarily rapid progress within the last few years; the labourers in its fields have been numerous and skilful, and the stores of knowledge they have accumulated have been proportionately great and valuable. To its more recent cultivators the British islands have furnished a fair quota, not least distinguished amongst whom, for labour, research, and original discovery, stands the name of our own countryman, the second edition of whose work we are now about to notice. To him are due many of the industrial applications of the science, more especially as regards the improvement and advancement of agricultural industry. While, however, Sir Robert Kane has applied his talents to the prosecution of this, probably the most directly beneficial of the many branches of so generally useful a subject, he has not neglected to cultivate what may be termed its higher walks. The first edition of his Elements of Chemistry, which was noticed at length in the eighteenth, nineteenth, and twentieth volumes of our former series, is the strongest evidence of this, now further confirmed by the improvements and additions made in the second edition just published. A few of these we shall proceed to mention shortly.

With reference to a work, the character of which is so well established, it might be almost sufficient to state that every chapter and every section have undergone such alterations, and received such additions, as render the entire a complete exponent of the present state of chemical science; but we cannot avoid pointing out some of the more important of these alterations and additions.

In the section on dynamical electricity the most recent improvements in the construction of constant batteries are noticed at length, especially those of Professor Callan and the gas battery recently invented by Mr. Groves. The exertions of the first-named natural philosopher, and his inventive skill, are testified by

his battery of 800 four-inch square plates, constructed on his own principle for the College of Maynooth, and which is described by our author as producing the most brilliant results, and being, probably, the most powerful galvanic apparatus now existing.

In the fifth chapter, that on chemical nomenclature, we are presented with a startling proof of the discoveries made by chemists during the last eight years. When the first edition was published in 1841, but fifty-five elementary substances, it was stated, were at that time known; but in this edition it is mentioned that "there are at present known sixty-two substances which the chemist has not been as yet able to separate into other elements."

The important subject of *thermo-chemistry* is most carefully considered in the seventh chapter; the determination of the quantity of heat produced during the combustion of a given quantity of combustible substance—an economical question of such vast importance in the arts—is elucidated by a valuable commentary on the recent researches of Grassi, Faber, Silberman, and Andrews. The investigations of the three latter chemists are especially noticed as negativing Welther's rule, that in all cases of combustion the quantity of heat evolved is proportional to the quantity of oxygen which enters into combination:

"Those chemists having determined the heat evolved by the combustion of a great variety of ethers and carbo-hydrogens, found by their results, that, on subtracting the oxygen, together with the hydrogen necessary to convert it into water, the remaining elements do not develope as much heat as they should in a free state. They also examined sulphur in its different forms, and found that it evolved 23 units of heat. In its combination of sulphuret of carbon, which yields 34 units, the elements, if free, would have given $31\frac{1}{2}$, that is, when combined they yield $2\frac{1}{3}$ more than when free."—p. 245.

Sir Robert Kane is inclined to coincide with Professor Andrews, of Belfast, in the results obtained in the experiments of that chemist, by which it was proved that the quantity of heat evolved by the union of acids and bases is determined by the nature of the base, and not at all by the nature of the acid,—a result directly contradictory to the views of Hess; and adds, that it is to the Belfast chemist "we are indebted for the philosophical announcement of the numerical laws of thermo-chemical action."

The additions to the sections on the atomic constitution of matter, and the isomorphism of bodies, are valuable and important. The opinions advanced by Kopp and Schroeder in Germany, and by Playfair and Joule in England, are carefully described; and as Sir Robert Kane does not consider that the

results detailed by these investigators have been very satisfactory as an addition to our knowledge, he proceeds to point out and correct the "inconsequences" into which they have fallen. Our author's views on this department of chemical philosophy are described in his usual lucid style; and although we are most anxious to make our readers acquainted with them, we fear that they might suffer by any attempt at abridgment on *our* part, and therefore prefer to recommend their perusal in his own words. In the first edition the circumstances of isomorphous replacement were reduced to six simple propositions. To these a seventh is now added, namely:

"We cannot admit that isomorphism results from equality of the atomic volumes of bodies, as we have found that, although many isomorphous bodies have the atomic volumes the same, many others have quite different atomic volumes, and bodies have the same atomic volumes which are not at all isomorphous."—p. 314.

That portion of the volume devoted to *organic chemistry* has been most carefully revised, and by the simplicity of the arrangement adopted, and the clearness of the details, stripped of much of its difficulty. The most recent improvements in the methods proposed for organic analysis are given at length, and the process of Will and Varrentrapp for determining the quantity of nitrogen is described and most deservedly noticed in terms of high commendation for its simplicity and rapidity in execution. The improved method of alcalimetry proposed by Will and Fresenius is also described in full, and its application to *acidimetry* pointed out. The nature of the organic bases, as determined by the artificial production of organic alkalies, and the constitution of the organic alkaloids, are investigated with much care; and a concise but clear account of the theoretical views now generally adopted, and which are founded on those advocated by Fresenius, is given.

The views as to their constitution, when based on Woehler's hypothesis, are thus clearly and concisely described:

"The idea of Woehler that narcotine consists of opianic acid united to the true alkaloid, and that it is analogous in constitution to opiammon, presents to us another view of the constitution of organic alkalies, and one recently supported by additional results. Thus Rochleder and Redtenbacher have announced that piperine consists of aniline, with two equivalents of an azotized acid, and they have produced it artificially from aniline. Also that narcogenine consists of the opianic acid, with two equivalents of the same base that exists in narcotine. This form of saline or binary constitution may exist very extensively in those bodies, and it is to be hoped that the researches now being undertaken in this highly important field will soon enable us to reduce the natural alkaloids

to the same principles of classification as I have shown to regulate the alkalies of artificial origin."

"In concluding this subject," writes Sir Robert Kane, "it is important to record, that for the researches on aniline, which were the basis of all subsequent exact inquiry on this branch, science is principally indebted to Professor Hoffman, with whom was associated, in many of his investigations, Dr. Muspratt, of Liverpool."—p. 976.

The chapter on the chemical phenomena of vegetation contains a most valuable summary of the present state of chemical knowledge, as it bears on agriculture. In it, the mode of nutrition of plants is described in simple language, and the action of manures, and the effect of the rotation of crops, clearly explained.

We have now mentioned a few, and but a very few, of the improvements to be found in the second edition of Sir Robert Kane's Elements of Chemistry. Every page bears the stamp of careful revision; and we need scarcely remark, that no new fact or novel theory has escaped the notice of the author. The entire work, now perfect in every department, constitutes a volume equally useful, we might say necessary, to the student of medicine and the physician, to the simple husbandman and the skilled agriculturist, to the man of letters and the scientific philosopher. Indeed we know of no work on the subject to be compared to it, or which combines so happily compendiousness and completeness.

Before concluding we think it necessary to remark that, although the number of pages in the present are fewer than in the former edition, it contains a much greater amount of matter, which has been effected by enlarging the size of the printed page, and by the use of a new and more condensed, yet clear and easily-read type. The woodcuts are beautifully executed, and many new ones seem to have been added.

Medical Portrait Gallery. By T. M. STONE, Esq., Librarian to the Royal College of Surgeons, London.

IT is only when we have lost an esteemed preceptor or a valued friend that we regret the want of a portrait or some such *souvenir*. We suppose the hearts of Englishmen are as retentive of early recollections, and as susceptible of emotions of friendship, as our continental neighbours; and yet there is scarcely a single professor, or man of any eminence, either connected with the universities of Germany or in private life there, whose litho-

graph may not be obtained in the adjoining Stadt. With the portrait of each favourite teacher every student provides himself on his obtaining his degree, and preserves it as a memento of his days of *Burchenschaft*. We have ourselves, as far as in our power, endeavoured to remedy this defect, and presented our readers with several portraits of Irish medical worthies during the last three years.

These observations are drawn from us by the receipt of five lithographic prints, a portion of a series of medical portraits now being produced by Mr. Stone, to whose zeal and energy we, in common with the profession generally, are much indebted. Those already published are of Professors Todd and Fergusson,—both capital likenesses,—and also Drs. Budd and Forbes, and Mr. South, all men distinguished in their several walks in the profession. They are skilfully drawn, and very well printed on India paper. We wish the undertaking every success, and soon hope to see the same enterprising spirit displayed by some artist and publisher in our own country, who shall, in the words of Mr. Stone in his prospectus, "furnish the members of the profession with the best remembrances of those who have attained for themselves a reputation in its ranks."

First Steps to Zoology. By Robert Patterson, Author of "Zoology for Schools." In two Parts, with 244 Illustrations. London, Simms and M'Intire. 1849. pp. 252.

Two charming little volumes, conveying sound instruction, while merely appearing to amuse; written in an easy, familiar style; and most copiously illustrated with wood engravings. We have not met with any other work so applicable to its intention, and so admirably fulfilling the objects of its author,—that of imparting to juvenile readers the rudiments of animated nature. Parents, teachers, and the rising generation, owe Mr. Patterson much for his successful efforts to render the latter acquainted with the forms, habits, and organization of animals.

If we have a fault to find it is that a proportionate size has not been preserved by the artist who illustrated the work: for instance, the figure of the little Stickle-back, or Pinkeen, of our pools and streams, is as large as that of the Sturgeon; and the representation of the huge Tunny fish nearly three times as small as the accompanying woodcut of the Pilot fish! These, for children, are objections. We have no other faults to find.

PART III.

REPORTS, RETROSPECTS, AND SCIENTIFIC INTELLIGENCE.

RETROSPECT

OF THE PROGRESS OF GENERAL, PHYSIOLOGICAL, AND PATHOLOGICAL CHEMISTRY, FOR THE YEARS 1846 AND 1847.

By WILLIAM SULLIVAN, Esq.,

FIRST CHEMICAL ASSISTANT IN THE MUSEUM OF IRISH INDUSTRY, AND CHEMIST TO THE ROYAL AGRICULTURAL SOCIETY OF IRELAND.

(*Continued from page* 255 *of No.* IX. *February*, 1848).

Owing to the limited space which must be assigned to a Report on Chemistry in this Journal, it would be impossible to notice even one-half of the important memoirs which have appeared within the period specified, and I have, therefore, confined myself principally to two divisions of the subject,—physical and animal chemistry. My object in so doing has been to direct attention, in the first place, to a branch of chemistry which is very little studied except by pure chemists, but which is yet the most important,—namely, the laws which affect chemical combination; and, in the second place, to turn, if possible, the attention of young physicians to a new and rich field of inquiry which can only be properly cultivated by their assistance. The Irish school of medicine has earned a well-deserved fame for practical knowledge, the true basis on which success in practice must always depend; but as yet it has, with a few honourable exceptions, not done much for purely *scientific* medicine. I have, therefore, collected together a few samples of what the continental professional men are doing in one department of this, in the hopes that it may induce some to imitate them, and thus assist in maintaining the reputation which their predecessors gained for the Irish school in other more practical, though not more useful departments. I have strictly confined myself within the years 1846 and 1847, in order to be able in future to continue in a systematic manner an annual report; and as I shall have henceforth to notice only the work of one year, I hope to be able to do more justice to the subject(*a*).

(*a*) I am indebted for many of the papers contained in this Report to Canstatt's *Jahresbericht der Gesammten Medicin*, one of the most able and complete

Atomic Volume, and Specific Gravity.—Playfair and Joule(*a*) state that the atomic volume of ice, 9.8, is a submultiple for a large class of salts; and that this number was itself made up of numbers due to the volumes of the constituents of which it was composed, $\frac{9.8}{8} = 1{\cdot}225$, which they consider as the primitive volume. In support of this view, they calculated the volumes of all the metals the specific gravity of which have been tolerably accurately determined, and found the result one-twentieth less than the theoretical expression. They ascribed this deviation to the force of cohesion in the metals; and in proof of this, they found the specific gravity in the state of fusion gave results in exact accordance with theory.

They examined the oxides and sulphurets, and found that they gave very exact results, the force of cohesion not being so powerful; but in those usually called the magnesian oxides, they found five and a-half unit volumes instead of five volumes. They therefore contended that the magnesian metals ought to have their equivalents doubled, and that their oxides instead of being

$$R_2O,\ RO,\ R_2O_3,\ RO_2$$

should be

$$RO,\ RO_2\ RO_3.\ RO_4,$$

the oxygen increasing in arithmetic proportion. They also examined the volumes of dimorphous and polymorphous substances, and found that the differences of volume were either expressed by the unit volume 1·225, or by a multiple of it.

Influence of Salts or Acids on the Decomposition of Water by Metals.—Millon(*b*) has studied this subject very accurately, and ob-

works of the kind ever published. I have also used occasionally the Chemical Gazette, and I may take this opportunity of recommending this excellent little journal to all interested in chemistry. All the most important papers published are immediately noticed in it, and the abstracts of them which are given are most admirable. I may also mention, as connected with the progress of chemistry, the formation of the Cavendish Society, the objects of which are, the translation of recent works and papers of merit; the publication of valuable original works, which could not otherwise be printed, from the slender chance of their meeting with a remunerative sale; and the occasional republication or translation of such ancient or earlier modern works as may be considered interesting or useful to the members of the Society. They have already published one volume of reports, among which is a paper of great interest to medical men, by Julius Vogel, on Endosmosis; and an admirable one on Isomorphism, &c. The Society is at present engaged in the translation of Leopold Gmelin's great work, the *Handbuch der Chemie*, of which they intend to publish one volume this year. As the power of the Society to publish, and consequently the value received by the subscribers, entirely depends on the latter, I think I shall be doing a great service to chemistry in Ireland by recommending every one who has an interest in the advancement of the science to become members of it.

(*a*) Memoirs of the Chemical Society; and Chem. Gaz., No. 82, p. 126.

(*b*) *Compt. rend.*, July, 1845; *Annal. de Chem.* vol. ix. p. 221; and Chem. Gaz., No. 83, p. 132.

tained some very important results. He found, for instance, that the rapidity of decomposition was sometimes increased one-hundred-fold, even by a minute quantity of certain salts. Thus pure zinc in dilute sulphuric acid is very little acted upon; but on adding a small quantity of certain salts the action is increased, and this increase differs for each salt. Thus if the action of pure dilute sulphuric acid is represented as 1, that of the bichloride of platinum will be 149; of arsenious acid, 123; of sulphate of copper, 45; and so on with other salts. A solution of oxalic acid has no action on zinc, even when heated, but with a few drops of chloride of platinum the whole is soon converted into oxalate. The action of a great many other acids is hastened in a similar matter.

This addition of a salt sometimes changes the result; nitric acid with four and a half equivalents of water, and diluted with two or three times its volume of water, poured on iron filings acts on the iron with evolution of nitrous acid fumes; but if only one drop of bichloride of platinum be added, hydrogen is evolved in place of nitrous acid, and protonitrate of iron with nitrate of ammonia is formed. Bismuth, silver, and mercury, do not decompose water under any circumstances. Copper, in contact with bichloride of platinum and dilute hydrochloric acid, heated, gives off hydrogen freely; but if nitric acid be employed, the same chloride will at once arrest the action. He obtained with other metals equally important results.

Influence of Light on Chemical Affinity.—Jules Bouis(*a*) found that if a boiling, saturated solution of cyanide of mercury be exposed to the action of chlorine gas in strong sunlight, it will be decomposed into chloride of mercury, hydrochloric acid, and sal ammoniac, which remains dissolved, and chloride of cyanogen, nitrogen, and carbonic acid, which are evolved.

Observations on the oxidizing Power of Oxygen when disengaged by means of Voltaic Electricity.—Kolbe(*b*) found that by submitting a concentrated solution of sesqui-chloro-carbo-hypo-sulphate of potash to the action of a voltaic battery it was decomposed into chlorine, carbonic acid, sulphuric acid, and perchlorate of potash, the formation of the latter being preceded by that of chloric acid. When chloride of potassium was substituted, chlorate and perchlorate of potash were also formed, even when sufficient sulphuric acid was added to combine with all the potash.

On the Decomposition of Water into its constituent Gases by Heat.—Grove(*c*) states that every process which combines gases will separate them. Thus he found that platinum wire heated almost to its fusing point decomposes water, giving off a current of mixed gases,

(*a*) *Annal. de Chim. et de Phys.*, vol. xx. p. 446.
(*b*) Chem. Gaz., No. 101, p. 27.
(*c*) Report of the Meeting of the Brit. Assoc. for 1846.

being exactly the reverse of what takes place when such a wire is introduced into a mixture of oxygen and hydrogen, water being in this case formed.

Amount of Ammonia in the Atmosphere.—Graeger(*a*) has found that 1·000000 part of air, the barometer being at 744·97^{mm}, and thermometer at 32° F., contains 0·6148 grms., or somewhat more than $\frac{3}{5000000}$ of carbonate of ammonia.

On the Presence of Hydrosulphuret of Ammonia in Hail.—Pelletier(*b*) found sulphuret of ammonium in the hail which fell at Douèla-Fontaine on the 26th and 27th of January, 1846.

On the Existence of a new Oxacid of Nitrogen, the Pernitric.—Barreswil(*c*) is of opinion that the blue liquid which is obtained on condensing by cold a moist mixture of hyponitric acid and binoxide of nitrogen is neither nitrous acid nor a mixture of binoxide of nitrogen and hyponitric acid, but a definite compound, corresponding to permanganic and perchromic acid, and having the probable formula of NO_7. It has not, however, been as yet obtained in combination with bases.

On the Formation of Hypo-iodous Acid.—Kœne(*d*) has succeeded in obtaining hypo-iodous acids, but not in the same way as hypochlorous acid.

Pentathionic Acid, a new Acid of Sulphur.—Wackenroder(*e*) has obtained a new acid by passing sulphuretted hydrogen through aqueous sulphurous acid, which he calls pentathionic acid. Its formula is S_5O_5. It is not altered by sulphuretted hydrogen, even after being kept for several months. Dilute sulphuric or muriatic acid produces no turbidness, but the hydrate of sulphuric acid decomposes it completely, sulphur being deposited. Chloride of barium is not precipated by it, but a white turbidness is immediately produced with a large excess of perchloride of mercury. Acetate of lead produces no precipitate; but with a solution of the barytes salt yields a copious white deposit, which is soluble in excess of the precipitant.

On a new Series of Acids of Sulphur.—Plessy(*f*) has obtained a new series of acids by the action of sulphurous acid on the perchloride of sulphur in the presence of water. If the salt S_3O_6BaO, 2HO, which he had previously described, be dissolved in water, it parts with sulphur, and a new salt is obtained, having the formula S_5O_7BaO, HO. The salt S_5O_6BaO, 2HO contains two acids, one of which, by parting with sulphur, yields the acid S_5O_7. This more sulphated acid has not, however, been isolated, but its formula may be assumed as S_6O_7.

(*a*) *Archiv. der Pharm.* vol. xliv. p. 35.
(*b*) *Compt. rend.*, March 2, 1846.
(*c*) *Comp. rend.*, Sept. 21, 1846.
(*d*) Poggendorff's *Annal.*, vol. lxvi. p. 302.
(*e*) *Archiv. der Pharm.*, vol. xlvii. p. 272.
(*f*) *Compt. rend.* Feb. 8, 1846.

On a new Acid of Sulphur.—Fordos and Gélis(*a*) have described an acid having the same composition as that of Wackenroder's, namely, S_5O_5, formed by the action of aqueous sulphurous acid on any of the chlorides of sulphur, but which they say cannot be compounded with chloride of sulphur, Wackenroder's acid, the barytic salt of which is soluble in alcohol and in ether, and cannot be precipitated from its aqueous solution by these agents, whereas this is exactly the method which they adopt to isolate their acid. This new acid is isomeric with hyposulphurous acid, S_2O_2, but they differ in all their characters. There is no such case of isomerism as this in mineral chemistry, we must look to organic chemistry for analogous cases; and this fact supports the opinion of Berzelius of the analogy between the composition of the recently discovered acids of sulphur and organic compounds.

Fordos and Gélis divide all the acids of sulphur into two classes: in the one the sulphur remains invariable, and the quantity of oxygen increases; in the other, the number of equivalents of oxygen being 5, it is the sulphur which varies as the numbers 2, 3, 4, and 5. The new acid is the last acid in the second class. They denominate the first the *sulphuric* series; it contains the acids formerly known; and the second the *thionic* series, which includes the four acids recently discovered. We have thus—

Dithionic,	S_2O_5,	hyposulphuric acid of Gay-Lussac and Welter.
Trithionic,	S_3O_5,	sulphyposulphuric acid of Langlois.
Tetrathionic,	S_4O_5,	bisulphated hyposulphuric acid, discovered by themselves in 1842.
Pentathionic,	S_5O_5,	the new acid.

It will be a very curious case of isomerism, indeed far more curious than any before noticed, if it shall be established that there are really two acids of sulphur, with the formula S_5O_5 as these experiments of Fordos and Gélis, and those of Wackenroder above mentioned, lead us to believe.

New Metals.—A number of new metals have been latterly discovered; among others, Dr. Claus discovered ruthenium in the residues of the platinum ores, that fruitful source of new metals. Herman discovered ilmenium in a new mineral called yttro-ilmenite; and Heinrich Rose has discovered two new metals called pelopium and niobium in the Bavarian tantalite.

On some double Salts of the Magnesian Group.—J. Isidore Pierre(*b*) has very carefully examined the salts of this group, including those of magnesia, copper, zinc, nickel, cobalt, manganese, and iron. Graham considers, with respect to the sulphates of these bases, that one of the equivalents of water cannot be eliminated except at a much higher temperature than is required for the others; that this equi-

(*a*) *Compt. rend.*, Nov. 1847; and Chem. Gaz. No. 124, p. 491.
(*b*) *Annal. de Chim. et de Phys.*, Feb. 1846; and Chem. Gazette, No. 82, p. 113.

valent may be replaced by an equivalent of a salt, so that the double salt formed contains one equivalent less of water than if each of the two simple sulphates had brought all its water into the molecule of the double salt resulting from their combination.

Pierre states that the results which he has obtained do not confirm those of Graham; he found that the sulphate of zinc, which contains 43·72 per cent., or seven equivalents of water, when exposed for a long time to a temperature of 230° F., and a current of dry air, lost 43·6 per cent., or the whole of its water; whilst Graham states that it requires a heat of 400° F. to expel the seventh equivalent.

The following are the general results at which he has arrived:

1. Sulphate of zinc, containing seven equivalents of water, retains the whole of it in the compound which it forms with the alkaline, or alkalino-earthy sulphates.

2. The sulphates of zinc and magnesia combine equivalent to equivalent, and the resulting compound contains a quantity of water equal to the sum of the quantities which both salts contained when separate, that is to say, fourteen equivalents, if the double salt crystallizes at common temperature.

3. The simple sulphates of zinc, copper, and nickel, yield all their water at a little above 212° in a long-continued current of air, instead of retaining one equivalent at 400° F., as stated by Graham.

4. The simple sulphates of zinc, magnesia, copper, and nickel, combine with other sulphates, or with each other, without elimination of the water.

Professor Graham has repeated his experiments, and still adheres to his original view of the constitution of the magnesian sulphates which these results of Pierre had impugned.

Researches on the Hydrates.—Fremy(*a*) has published some very interesting remarks on this subject, but which our space forbids us from noticing at the length which they deserve. Graham, in his investigations on phosphoric acid, has very clearly shown that there exists, in many cases, a certain relation between the capacity of saturation of an acid, and the quantity of water which this acid retains when it is hydrated. Fremy has also shown that the capacity of saturation of the tartaric and paratartaric acids decreases in proportion as they become dehydrated. But when acids become completely dehydrated, have they lost the property of combining with bases,—can they, in fact, be considered still as acids? This question, so important to chemical statics, Fremy first examines.

The property which acids possess of combining with bases in order to saturate them, and of displacing other acids from their saline combinations, according to the principles of Berthollet, must be considered as their essential character. Fremy, therefore, in order to determine the acidity of the anhydrous acids, caused them

(*a*) *Compt. rend.*, Dec. 21, 1846.

to act upon anhydrous bases, or upon salts containing acids of a less fixed nature. To this double test he submitted nearly all the anhydrous acids known,—as the carbonic, sulphurous, sulphuric, phosphoric, silicic, boracic, stannic, &c. These experiments prove that these acids may present all the characters of acids without the intervention of water, that is, that in the anhydrous state they are still true acids. Having established this point, Fremy wished next to ascertain whether all the acids were capable of forming anhydrous salts with bases, and whether there does not exist a certain number of salts in which water was an indispensable element.

It is generally admitted that the water of crystallization contained in the salt is outside of the saline molecule, and that consequently it only influences the physical properties of the salt. Recent experiments, however, prove that this is only partially true; Wurtz, for instance, has proved that the phosphites and hypophosphites contain a certain quantity of water, of which they cannot be deprived without complete decomposition. Chevreul has always arranged in different species the salts formed by the same acid and the same base, but which contain different proportions of water; and Regnault has long considered the water of crystallization as forming, with the anhydrous salts, groups which cannot be modified without at the same time producing a considerable change in the properties of the salts. These views are fully confirmed by the experiments of Fremy, who shows that there are several classes of salts, the molecules of which are truly ternary, and are always formed by the combination of an acid, a base, and water.

Fremy imagined that if the powerful acids, such as the sulphuric, nitric, &c., form with equally powerful bases salts which are almost constantly anhydrous, this would not be the case with those acids which possess but a weak affinity for bases. His attention was therefore directed to those hydrates of the metallic oxides which are soluble in alkalies, and which may be regarded as weak acids; and he found that these bodies have the property of entering into combination with bases only in the state of hydrates. Among the oxides which he examined were those of copper, tin, antimony, and chromium, which, according to his experiments, are represented by the formulæ:

$$CuO2, HO;\ SnO, HO;\ SbO_3, HO;\ Cr_2O_3, 10HO.$$

They dissolve in alkalies, and become insoluble on losing their water. Fremy convinced himself that the insolubility of these oxides in alkalies is really due to the elimination of a certain quantity of water, and not to those isomeric modifications which certain oxides experience when ignited.

The examination of the stannic and antimonic acids furnish him with striking examples of hydrates. He considers that these experiments establish a marked difference between the two stannic acids. Metastannic acid, he states, is capable of forming three hydrates with water. The neutral metastannates formed in the presence of a large excess of alkali, he represents by the general formula $Sn_5O_{10}, MO, 4HO$,

while he represents the stannates by the formula SnO_2, MO; the equivalent of metastannic acid being thus five times higher than that of stannic acid. He has, moreover, established the fact that the metastannates are always hydrated, that they are necessarily ternary, and are decomposed when deprived of their water. In this case the metastannic acid loses its acidity, while the stannates may be reduced, like most salts, to the anhydrous state without being decomposed. The fact that the same oxide is able, by combining with different proportions of water, to form two acids which are distinguished from one another by their properties, their equivalents, and their mode of combination with bases, is certainly very important, but it is by no means isolated.

The antimonates form a class of salts of great interest with reference to their water. Fremy found that each antimonate could combine with water in several proportions, and form ternary groups, a kind of double salts which cannot be compared to the ordinary hydrated salts, for each group possesses special chemical reactions which differ in proportion as the salt loses its water.

Conversion of Nitrates into Salts of Ammonia.—Kuhlmann(*a*) some years ago showed, in a beautiful series of experiments, that the formation of nitric acid in nitre heaps was preceded by the formation of ammonia. He also showed that the production of ammonia by the action of weak acid upon tin was not an isolated fact, but that it resulted from the action of this acid upon all the metals capable of decomposing water, and consequently the ordinary result of the contact of nascent hydrogen with nitric acid. Fordos and Gélis have confirmed all his results, and have added several other interesting facts, among others the decomposition of sulphurous acid by the action of nascent hydrogen.

Kuhlmann has since added some further experiments, in which he shows many interesting cases of the conversion of nitrates into ammoniacal salts. He found when some fragments of nitre are thrown into a mixture of zinc or iron and sulphuric acid, or, what is preferable, weak hydrochloric acid, the disengagement of hydrogen is stopped or retarded until the whole of the nitric acid of the nitrate is converted into ammonia. Nascent sulphuretted hydrogen occasions a similar conversion, at the same time depositing sulphur. When a nitrate, the metal of which is precipitated by zinc,—nitrate of copper for instance,—is placed in presence of a mixture of zinc and hydrochloric acid, for every equivalent of zinc dissolved there is one equivalent of copper precipitated, and one equivalent of the nitric acid of the nitrate is converted into one equivalent of ammonia, the whole without any liberation of hydrogen. By passing a current of sulphuretted hydrogen through a solution of chloride of antimony mixed with a nitrate, the acid of this latter is converted into ammonia. Similar metamorphoses are effected by the contact of nitrates with

(*a*) *Compt. rend.*, Nov. 30, 1846.

a solution of sulphuret of arsenic in potash, or with the hydrated protoxides of tin and iron.

I shall refrain from noticing any of the numerous memoirs on vegetable organic chemistry which have appeared during the years 1846 and 1847, as even a list of them would occupy more space than can be devoted to the whole Retrospect. I do this with the less regret as my object has not been by any means to give a complete report on the progress of chemistry, but rather to point out to medical men the direction in which chemistry tends, as a science; to show them, in fact, that it no longer consists of the history of oxygen and hydrogen, and pharmaceutical preparations,—that, in fact, it will shortly be included within the domain of mathematics; and, on the other hand, to show them that in proportion as it has left the shop of the apothecary it has entered the hospital, and is fast becoming the most useful instrument in the hands of the physician.

ON PHYSIOLOGICAL AND PATHOLOGICAL CHEMISTRY.

Physiological Physics.—*Endosmose and Exosmose.*—Matteuci(*a*) has published another work in which he repeats the general views already put forward in his previous works, and also added a great many new views and experiments, especially on endosmose and exosmose, a few of which I shall notice here, as they are probably not generally known to most of my readers.

If a number of tubes with fine bores be immersed in a fluid it will rise in them exactly proportional to the size of the bore; the finer the bore the higher the fluid will rise. This action is called capillary attraction, and upon it depend a great variety of phenomena, not only in inorganic nature but also in vegetable and animal bodies, such, for example, as the effects known as imbibition, exosmose, and endosmose. In most animal and vegetable organs the structure resembles in effect a bundle of such tubes from the small spaces which exist between the parts. As the phenomena depend more on the nature and density of the fluids than on the substance of the tube, different fluids will ascend in the same tube, under otherwise similar circumstances, to an unequal height. If the blood happens to be altered in disease, so as to be rendered thicker or thinner by an increase or diminution of the fibrine or blood globules, or thinner by bleeding, or if it is modified by medicines, its capillary action will be changed,—water, oil, spirits, soup, &c., will disappear from the stomach quicker or slower than in a healthy natural state. The absorption of a liquid will take place more rapidly the higher its temperature is. This absorption or imbibition is not influenced by the pressure or by the moisture of the atmosphere; it produces heat and electricity; and as it plays a very important part in the capillary motion of the blood, it is probable that it has some influence on the development of the chemical activity of organic bodies. The cause of capillarity is the opposition of heterogeneous and ho-

(*a*) *Leçons sur les Phenomenes Physiques des Corps Vivants.* Paris, 1846.

mogeneous attraction between a fluid and the solid tube. Endosmose and exosmose on the other hand depend on the mutual attraction of two fluids, one of which is more capable than the other of freely wetting a porous solid which forms part of the combination. Thus if we dip a piece of bladder or other animal membrane into water it will be wetted and completely soaked with the liquid, but if we dip it into spirit it will not be wetted. If we take a funnel to the stem of which a long tube is attached, and cover the mouth with a piece of bladder or other animal membrane, and then fill the funnel with spirits of wine, and place it with the bladder downwards in water, the water will pass into the funnel and mix with the spirit, and a column of liquid will be forced up the tube. The moving power in this case is the force of adhesion between the water and the bladder; the water ascends through the pores of the bladder as it would ascend through any other tubes, and comes in contact with the spirit, with which it unites and is at once removed, and this process goes on for a certain time; the height of the column of liquid in the tube being in some degree an index of the power called into action. Other fluids having a strong tendency to unite with water may be substituted with the same effect, and even solutions of different solid substances, or in fact any two liquids of different densities. In case both the fluids thus placed in juxta-position are capable of wetting the membrane or other porous diaphragm interposed there will be a flowing in as well as out, and hence the names which Dutrochet first gave the phenomena, of exosmose or flowing out, and endosmose or flowing in. Gases are also capable of exhibiting this phenomena, with the exception that in this case there is no limitation, as they are capable of unlimited diffusion through each other's mass. Matteuci applies this development of force to the explanation of a great variety of phenomena; among others he mentions the experiments of Poisseuille's on the purgative and astringent action of medicines. He observed in drinking Seidlitz water that an endosmose of the serum of the blood took place with these waters, and with glauber salts, sea water, and other saline solutions, through the animal tissues. This is exactly what occurs in all cases of purging produced by such salts, the endosmose of the serum and the solution of the salt in the intestines. In such cases a quantity of the serum is carried off by the bowels, and the rapidity of the action is in proportion to the strength of the solution.

Muriate of morphia added to saline solutions diminishes the endosmose of the serum with saline solutions in the intestines; hence its action in diarrhœa.

Absorption is not merely an imbibition of a fluid by the tissues with which it is in contact, but a transfer of the fluid to the blood-vessels as in the experiment with the spirit mentioned above. It is in fact a true case of endosmose and exosmose, and will be facilitated by the greater number of vessels which may exist in the part; hence the lungs are better adapted for it than the cellular tissues, and the skin least; hence, also, the rapid absorption of ether and

chloroform through the lungs. Absorption in general depends on the mass of the fluid already present in the body,—the more present the less is the absorption; it also changes with the temperature,—warm drinks are more rapidly absorbed than cold. The rapidity with which the fluid moves also facilitates it.

Exhalation is the opposite of absorption, and like it depends on the permeability of the vessels and on the mass of the liquid; is stronger in dry than in moist atmospheres, and frequently diminishes or increases tenfold. Imbibition, capillarity, and even molecular attraction, may overcome chemical affinity, and hence the organs of secretion cannot be looked upon as simple filtering apparatuses. Matteuci treats digestion and respiration in a similar manner. He also treats of the similarity and points of difference between electricity and the power of the nerves, and on several other points, upon which he, however, has communicated nothing new. The importance, however, of the general subject of endosmose renders it necessary to give a general sketch of the mode in which he applies it to explain phenomena.

Julius Vogel(*a*) has also published a very able memoir on the subject, in which, after having given a general description of the phenomena, he proceeds to apply it to the explanation of various animal functions, such as digestion, the secretion of sweat, of tears, of mucus, of milk, of saliva, of bile, and of urine. The length of the memoir, and the impossibility of condensing it within the limits which I could assign to a single essay, prevents me from giving an analysis of its contents, and I must consequently refer the reader to the original memoir, which has been fortunately translated into English and published by the Cavendish Society.

Magnetism. — Weber's(*b*) experiments on the action of the magneto-electrical current on the vessels and on the circulation of the blood show, that by the action of such a current the arteries of the extremities are diminished one-third or one-half their diameter, and that by its continued action they may contract to one-fifth or one-sixth, and cause the circulation to entirely cease. The contractions take place only after some time, but it continues even after the stream has ceased. When a weak current is employed the effects soon cease, but when a very strong one is used the artery loses its power of contraction, and an aneurism results. No certain results were obtained by its action on capillaries of one-ninety-sixth of a line in diameter, and it only produced very slight contractions in the veins. The action of the stream produced coagulation of the blood and cessation of the circulation most readily in the capillaries.

Action of Caloric.—Weber (E. H.)(*c*) found that cold diminishes

(*a*) *Die Geseze der Mischung der Flüsigkeiten und ihr Eindringen in permeable Subzanen.* Gottingen, 1846; also translated in the Memoirs of the Cavendish Society.

(*b*) *Wirkung d. Magnetelectrischen Stromes auf die Blutgefäse, Müller's Archiv.* Heft. 2 and 3.

(*c*) *Froriep's Notiz.*, 2 Reihe, Bd. 46, p. 69.

the ciliary motion, and that heat increases it. He also found(*a*) that a heat of about 124° F., and a cold of about 32° F., blunted the nerves of the tongue for some time, so as to destroy the sensation of taste; the organs of taste lose at the same time the power of distinguishing between heat and cold.

Electrical Endosmose.—Namias(*b*) states that when a positive current of electricity is passed through a liquid it produces a corresponding current in the liquid.

Respiration.—Lassaigne(*c*) gives us the result of his experiments on the carbonic acid contained in the atmosphere of confined buildings, that, owing to the law of the diffusion of gases, the carbonic acid does not exist in larger quantities near the floor, but is pretty equally diffused through the whole mass, and that, consequently, ventilation must have reference to the whole mass of air in a room, and not to a particular part.

Letellier(*d*) has found that all animals expire, at 0° F., double as much carbonic acid as at 30°; and between these extremes it diminishes as the temperature increases.

Becker(*e*) has published some observations upon Scharling's experiments upon respiration, in which he impugns Liebig's opinion that oxygen combines more readily with alcohol when it exists in the blood, than it does with the decomposed tissues; and comes to the general conclusion that the use of alcohol retards the formation of living tissues, and considerably diminishes the consumption of oxygen, and consequently of carbon. He also states that a healthy person expires, as carbonic acid, at least one-third more carbon than Liebig states; and this he accounts for by Liebig not calculating, in his tables, the amount of beer and spirits consumed by the persons on whom he experimented. He also considers that the amount of nitrogen which is daily given off is much greater than is usually supposed. In conclusion, he states that the smoking of tobacco diminishes the quantity of blood conveyed to the lungs, and at the same time induces the same conditions in the blood as the ordinary narcotics, and consequently retards the metamorphosis of the tissues, a fact which he quotes against Scharling, Liebig, and Vierordt's results, as they did not take this point into account.

Incubation.—Gobley(*f*) has made the interesting discovery that the yolk of the egg contains phospho-glyceric acid, an acid already obtained by Pelouze from the action of phosphoric acid on glycerine. He also obtained the albuminous substance termed by Dumas and Cahours, vitalline. Among the other substances which he obtained were margaric and oleic acids, which, as well as the phosphogly-

(*a*) Müller, *Archiv.*, Heft. 4. (*b*) *Froriep's Notiz.*, Bd. 40, p. 263.
(*c*) *Bulletin de l'Academie de Medicine*, t. xi.; also *Comp. rend.*, t. xxiii. p. 188.
(*d*) *Froriep's Notiz.*, vol. xxxviii. p. 154.
(*e*) *Heller's Archiv.*, Heft. 5, p. 404.
(*f*) *Recherches sur le Jaune d'Oeuf; Jour. de Pharm. Annal. de Chim.*, &c.

eric acid, were combined with *ammonia*, margarine, oleine, cholestearine, chloride of ammonium, traces of lactic acid and iron. To these, Kodweis(*a*), who worked upon the same subject some years ago, adds stearic acid.

Sugar of Milk in incubated Eggs.—Winckler(*b*) has detected sugar of milk in incubated eggs.

Seraf. Cappezzuoli(*c*) has instituted some experiments to determine the quantity and quality of the substances assimilated during the development of the egg, and immediately after the chick is disengaged from the shell. His results are as follows:

1. During the incubation and the first few days of the existence of the chick, after leaving the shell, the quantity of fat does not diminish as rapidly as the albuminous substances.

2. During the incubation, the total diminution is very small, but becomes remarkable if the chick is left without food for some time after its expulsion from the shell.

3. Chickens fed on starch only, exhibit a diminution of their fat, and even when fed in the ordinary manner, a considerable diminution of fat and of albuminous substance was perceptible, from which it may be concluded that the animal consumes more in the first days of its existence than it assimilates.

Baudrimont and Martin Saint-Ange(*d*) have published a very important and complete investigation of the chemical appearances and changes which the egg presents during its incubation, from which the following results may be deduced:

1. Eggs diminish in weight during their incubation, as was already well known.

2. Atmospheric air containing a certain amount of moisture, and a moderate temperature, are absolutely necessary.

3. They absorb oxygen, and at the same time evolve water, carbonic acid, nitrogen, and some sulphur compound, not further known.

4. The loss of weight sustained by the eggs is always smaller than the sum of the water, carbonic acid, and nitrogen expired; it is even smaller when the carbonic acid is reckoned as carbon, which shows that the oxygen of this acid is derived from the air.

5. The oxygen necessary during incubation is partially absorbed and partially employed in the formation of water and carbonic acid.

6. The volume of absorbed oxygen is exactly equal to that contained in the evolved carbonic acid; and the volume of evolved nitrogen is about one-half of the latter, or about a fourth part of the oxygen absorbed.

7. The fatty bodies are diminished in the egg during the time

(*a*) *Liebig's Annal.*, vol. xxxix. p. 261.
(*b*) Heller's *Archiv.*; and Chem. Gaz., No. 90, p. 280.
(*c*) *Polli's Annali di Chimica*, December, 1846.
(*d*) *Recherches sur les Phenomènes Chimiques de l'Evolution embryonaire des Oiseaux et des Batraciens; Annal. de Chim. et de Phys.*, October and November, 1847.

of incubation, and at the same time the nitrogenous substances are altered in constitution, as is easily seen by the evolution of nitrogen.

8. The absorbed oxygen and the expired carbon and nitrogen stand in definite proportions to one another, and can be expressed by the following formula:—$8O + 2C + N$, which gives $4O + 2CO^2 + N$. If the oxygen be partially employed in the formation of water, we would have $4HO + CO_2 + N$, of which $H_4\ C_2\ N$ would have been derived from the egg.

9. The incubated egg is richer in oxygen than the fresh.

10. The inorganic elements can change considerably in their nature and relative proportions, as well as in physical characters.

11. The phenomena attending the development of the embryo exhibit a perfect example of nutrition, in which the nutriment is not assimilated until it has been exposed to the action of oxygen.

Proteine, Albumen, &c.—From the great number of memoirs published on these bodies, and from the important discussions to which they have given rise, particularly between Mulder and Liebig, I find it would be at present impossible to do justice to the subject, and shall, therefore, defer until the next number any notice of them, when I shall have more space at my command.

Blood.—Dumas's(*a*) investigations were principally directed towards ascertaining the relation of the blood globules to solutions of salts, and to the method of obtaining them pure, and also to their composition.

His process for estimating the blood globules is founded on the fact that they are not decomposed by a solution of sulphate of soda when exposed to a full current of air so as to maintain them of a full arterial colour. He first deprives the blood of fibrine in the usual manner, mixes it with three or four times its volume of a saturated solution of sulphate of soda, and throws the whole on a filter, care being taken that as thin a layer of blood globules as possible lies on the filter, otherwise they will not be fully oxidized, and will assume a purple colour, and will then dissolve in the fluid. During the washing of the globules, he causes, by a proper apparatus, a stream of air to pass over them through a fine tube reaching nearly to the bottom of the filter, the fluid being at the same time supplied as fast as it flows through it. In this way, in a few hours, five to six grammes of pure blood globules may be obtained.

He found that all alkaline salts possessed the same properties as the glauber salts, such as phosphate of soda, &c., as also salts of organic acids. On the other hand, chlorides of sodium and potassium appeared to act differently, for if blood be mixed with these solutions and then exposed to the action of oxygen, it will remain of a dark violet. Sal-ammoniac acts in a similar manner. Dumas remarks that there is probably some connexion between this action and the commonly received supposition that scurvy is produced by the use

(*a*) *Recherches sur le Sang., Compt. rend.*, t. xxii. p. 900.

of salted meat, and also on the injurious action of sal-ammoniac on the blood.

It is worthy of remark that those substances which leave the blood the power of becoming arterialized by oxygen are exactly those which do not injure the blood globules, and by the use of which a colourless serum can be obtained.

Dumas has come to the general conclusion that the colouring matter of the blood has the power of assuming the peculiar arterial tint only so long as it exists in the form of globules, and that it loses this property when the globules are dissolved or changed, and that the colouring matter itself undergoes at the same time some alteration.

Dumas made comparative experiments with different portions of the same blood, from which he draws the following conclusions:

1. That the salts of the complex organic acids, such as tartaric and citric, &c., have less influence in preserving the integrity of the blood globules than salts with mineral acids.

2. That the salts with soda as a base preserve them better than those with potash or ammonia. It thus appears that between the integrity of the blood globules, the arterial condition of the blood, the phenomena of respiration, and the nature and the quantity of the salts of the blood, there is an important connexion.

Marchand(*a*) has examined the blood in reference to the statement of Dumas, that the blood globules are capable of a separate respirative function outside the body, and has come to the conclusion that that statement is incorrect.

Dr. E. Harless(*b*) has made some very beautiful experiments on the action of gases on the blood globules, from which it results that the blood undergoes a series of changes under their influence analogous to those produced by salts. He exposed a layer of blood globules under the microscope to the action of different gases, by means of a peculiar contrivance. The constant result obtained was, that when the diameter of the blood globules of the frog, from treatment with oxygen, had become 0·011, and the width 0·007, by the action of CO_2, the first was increased to 0·014, and the latter to 0·009. Oxygen also made the contours dark and sharp, and the form a long oval with flattened poles, the membrane was granulated; carbonic acid made the contours round or oval, the granulations disappeared: resting on their edges they exhibited a considerable projecting convexity, which gradually disappeared towards the poles. Finally the structure of the membrane was rendered perfectly clear by CO_2, and by long-continued exposure to it the sac completely dissolved. Harless considers these charges to be the result of disturbances in the endosmatic and exosmatic currents.

From these facts it would appear that the cause of the change of

(*a*) Erdmann's and Marchand's Journal, Bd. 38, p. 273.

(*b*) *Inaugural abhandlung über den Einfluss der Gase auf die Form der Blut Körperchen von Rana Temporaria.* Erlangen.

colour of the blood was principally physical, was in fact produced by the absorption of the gas by the blood, and the change in the form of the globules, an opinion which Marchand has also come to.

Bonnet(*a*) has re-discovered a fact long since observed by J. Müller, that if one-half part of blood be directly received from a vein into a solution consisting of one part of sugar-syrup, and three of water, it may be filtered without coagulating, and that in some time the filtered liquid deposits a gelatinous coagulum of fibrine, similar to, but not so consistent as that of blood in inflammation. Before this coagulation took place he tried the effect of various substances on the mixture with the following results:—

1. Weak solutions of pure or carbonated alkalies; solutions of different neutral salts, as saltpetre, common salt, iodide of potassium, sulphate of soda, &c.; decoctions of vegetable substances, containing much tannin, &c.; very weak solutions of acids, as sulphuric acid, &c.; all alkaline or acid animal fluids, such as decomposed ammoniacal pus, urine, gastric juice, sour milk, &c.; do not allow the coagulation to take place.

2. Neutral animal fluids, such as fresh milk, solutions of very violent vegetable poisons, as acetate of morphia, belladonna, and even solutions of arsenious acid, had no influence on the coagulation of the fibrine or of the blood. He then divides those substances into four classes:—

1. Such as alter neither the blood globules nor the fibrine.

2. Such as act on both, namely, dissolve the globules, and prevent the coagulation of the fibrine.

3. Such as dissolve the globules, but have no action on the fibrine.

4. Such as retain the fibrine in solution, but do not destroy the globules.

He considers that a great many conclusions may be drawn from these results, as to the mode of action of these bodies as therapeutical agents.

Poggiale(*b*) has published the results of a number of analyses of the blood of different animals, from which nothing new can be deduced. He has also published an analysis of the blood of a new-born infant, from which he concludes that the blood of the placenta is similar in composition to the blood of the fœtus; that the proportion of solid matter in the fœtal blood is greater than that in the blood of adults; that the blood of a new-born infant is rich in globules and poor in fibrine; that the quantity of albumen and fat is the same as in adult blood; that the iron is in greater proportion.

On the Existence of Carbonates in the Blood. — Liebig(*c*), from some well-conducted experiments, has come to the same conclusion

(*a*) *Lettre à M. Dumas sur le Sang.; Annal. de Chim. et de Phys.*, t. xxi. p. 189.

(*b*) *Recherches Chimique sur le Sang., Compt. rend.*, t. xxv. p. 110.

(*c*) *Annal. der Chem. und Pharm.*, Bd. 57, p. 126.

as Enderlin, that there are no carbonates in the blood of oxen. He did not find either sugar or urea.

Marchand(*a*) has come to the same conclusion as Scherer, that the opinion of Enderlin as to the existence of tribasic phosphate of soda ($3\,Na\,O + P\,O_5$) in the blood is incorrect. With reference to the conclusion of Liebig quoted above, he states that he has repeated his experiments with the same result, but does not, however, come to the same conclusion. He accounts for no evolution of CO_2 having been observed in Liebig's experiments, by supposing the greater part of the gas set free on saturating a small quantity of a carbonate dissolved in a large quantity of water, to have been absorbed, and consequently no effervescence produced, and in proof of this he brings forward several conclusive experiments, and among others shows that a fluid containing $2\,Na\,O, H\,O + P\,O_5$ in solution, when saturated with carbonic acid, scarcely lost any even under an air-pump. Marchand, therefore, adheres to his former opinion, that the blood contains carbonates, an opinion which is fully confirmed, at least in the case of the blood of the Herbivora, by the beautiful and conclusive experiments of Lehmann(*b*). Heinrich Rose(*c*) appears also to consider that carbonates may exist in the blood.

Urea in normal Blood.—Dr. Strahl(*d*) states that he has been able to detect urea in normal blood.

Pathological Chemistry of the Blood.—Dujardin and Didiot, Surgeons to the Hospital of Val de Grace, at Paris, have applied Dumas's method of examining the blood globules to diseased cases. In thirteen slight cases of typhus, the blood globules remained on the filter, or only a small portion went through. In seven severe cases, notwithstanding a strong current of air, they went through here and there, or in the form of red streaks; and this appearance of dissolution in the blood globules appeared to be in direct proportion to the violence of the disease. The globules behaved themselves similarly in spontaneous erysipelas, phthisis, many organic diseases of the heart, and typhoid inflammation of the lungs; while in pleurisy, simple pneumonia, acute catarrh, acute rheumatism, the serum passed through clear and without any globules.

Plethora, Anæmia, &c.—Becquerel and Rodier(*e*) found the chemical constitution of the blood in plethora almost normal. In simple chlorosis they also found that the composition of the serum was normal, and that the change which takes place in the blood of patients labouring under this disease is confined to the blood globules. On the other hand, in symptomatic anæmia the albumen of the serum is considerably diminished.

Sir Henry Marsh(*f*) has given, in his admirable paper on chlo-

(*a*) *Journal für Prak. Chem.*, Bd. 37, p. 321.
(*b*) *Journal für Prak. Chem.*, Bd. 40, p. 133.
(*c*) Chemical Gazette.
(*d*) *Preuss. Vereinzeitg*, No. 47.
(*e*) *Gaz. Med.*; the whole memoir is contained in Nos. 26, 27, 32, 33.
(*f*) Dublin Quarterly Journal, No. IV. November, 1846.

rosis, some chemical observations on the blood in that disease, from analyses by Dr. Hill, which tend to show that the disease does not consist in a diminished quantity of blood, but in an altered quality. In chlorosis, in fact, the blood undergoes a very remarkable change: its specific gravity is lowered, the clot is small and firm; the serum bears too large a proportion to the crassamentum; water is in excess; the red corpuscles are far below the healthy standard in quantity; their appearance, however, under the microscope, is natural; and the fibrine, in the majority of cases, is normal.

In hæmorrhagic diseases, on the other hand, the specific gravity is seldom much below, and occasionally even exceeds the healthy standard. The clot bears too large a proportion to the serum; the fibrine is less than in healthy blood, or rather its proportion to the globules is less than in healthy blood; the quantity of red globules is either absolutely increased, or their proportion to the fibrine is larger than in normal blood. The quantity of solid constituents frequently exceeds that of the normal fluid; a buffy coat is scarcely ever observed except where fever or inflammation is present.

Inflammation.—Zimmermann(*a*) makes the important observation, that in pneumonia, if the patient be bled on the first day of the disease, and that no increase of the fibrine is observed, its increase afterwards cannot be assigned as the cause of the disease.

Intermittent Fever, Dysentery, Typhus.—Leonard and Foley(*b*) have published a most excellent memoir under the title given below, on the composition of the blood in different diseases. They adopted in their analyses the process of Andral and Gavarret, and their results can, therefore, be compared. We can only give a few extracts, showing the results at which they arrived with reference to each of the constituents.

Fibrine.—In febris intermit. and remit. simplex, the mean quantity of fibrine was 2·5. Andral and Gavarret found, as a mean, 3·4. The quantity appears to diminish in repeated attacks of fever, but yet not to fall below the minimum normal state. During the change of the intermitting type into the remittent, no constant change in the quantity could be observed. In a complication of intermitting fever with pneumonia, the quantity of fibrine in the latter was increased. In violent cases the fibrine did not appear to be changed, and consequently it is not much influenced, except in a few cases, by inflammation. The frequent occurrence of hypertrophy of the spleen does not always depend, as Andral thought, on the diminution of the fibrine. The use of sulphate of quinine, to the amount of sixteen grains daily, for several days, had no influence on the quantity of fibrine.

Blood Globules.—They took 152 as the physiological maximum, according to Becquerel and Rodier, and as physiological minimum,

(*a*) *Uber die Analyse des Blutes.*

(*b*) *Recherches sur l'Etat du Sang dans le Maladies endémiques de l'Algérie; Mem. de Med. Chirurg., &c., Militaires*, t. lx. pp. 135 to 209.

110, according to Andral and Gavarret. As a mean of all cases (62) they found 111; as a mean of recent cases, 113·2; as a mean of relapses, 108·8. It results from this that, with the duration of the disease, the blood globules diminish.

Solid Constituents of the Serum.—In thirty-two cases out of sixty-two, these were within the physiological maximum of 91 (Andral and Gavarret), and the physiological minimum of 68 (Becquerel and Rodier); in ten cases it exceeded, and in twenty it sunk below the minimum.

They also investigated each constituent of the serum. The organic constituents exceeded the mean (72 A. and G.) in eighteen cases, and fell below it in thirty-three. The inorganic exceeded the mean (8) eighteen times, and fell below thirty-two times. In the complicated cases, the diminution was almost constant. In intermittent fever, the albumen of the blood was also in general diminished. In many of these cases the urine contained no albumen, which is contrary to the opinion of Andral, that a diminution of the albumen only takes place in those cases where it is carried off by the kidneys.

The constituents of the serum soluble in boiling water (extractive matter, soluble salts) were always increased. The authors believe, from these facts, 1st, that the fibrine of the blood does not increase at the cost of the albumen, and *vice versâ;* 2nd, that the blood globules do not increase at the cost of the albumen; 3rd, that the quantity of free salts of the blood, namely, the alkaline salts and free alkalies, do not appear to have any influence on the quantity of fibrine and albumen.

Water in the Blood.—Their researches on this point led to the following results:—In the cases where the quantity is increased, the globules are generally diminished. The solid constituents of the serum are also either diminished, or their quantity is normal.

When the water is normal, the blood globules are also generally so, and also the solid constituents of the serum, although there are greater proportional variations in the latter. Diminution of the water takes place very seldom, and always occurs with the increase of the globules, while, at the same time, the solid constituents of the serum are likewise either increased or remain normal.

The general results to be drawn from these investigations may be summed up as follows:—The blood during intermitting fever undergoes some change from which a series of secondary appearances, such as anæmia, dropsical and scorbutic attacks, are easily explained. The diminution of the globules (probably also a diminution of their power of absorbing oxygen) explains the prostration of the entire constitution and the occasional disturbances of the circulation. The diminution of the fibrine explains the ecchymosis in the skin and in the cellular tissues, the bleeding from the nose and gums, the gangrene of the mouth which sometimes occurs, and the pains in the limbs. The diminution of the albumen explains the hydropsical swellings, anasarca, ascites, and probably also the watery diarrhœa, which

always closes the last scenes of persons weakened by fever. These changes of the blood are not peculiar to intermittent fever, as they also occur in many other diseases.

Dysentery.—They examined the blood in six cases of this disease. They were principally those in which inflammatory symptoms predominated with violent fever, heat, headach, &c. In all the bleeding was serviceable. The fibrine was in general increased, the globules had a tendency to increase; the solid constituents of the serum were never increased, being generally normal; the organic constituents had a tendency to diminish; the inorganic were normal; the albumen, in the three cases in which it was determined, was diminished. The constituents soluble in boiling water, in the four cases in which they were determined, were considerably increased. The constituents soluble in boiling alcohol, as in the fever cases, gave no definite results. The water was four times increased and twice normal.

Typhus.—Becquerel and Rodier, from their investigations of the serum of blood in typhus, come to the following conclusions:—at first it differs very little from the normal, but during the course of the disease its density diminishes, both from the influence of the disease and the diet, and the quantity of water increases and the albumen diminishes. In violent cases of typhus the blood globules lose the property of absorbing oxygen.

Puerperal Fever—Hersent(*a*) draws the following conclusions from his researches:—

1. In cases of violent puerperal fever the quantity of water is very much increased, and the blood globules and albumen diminished. 2. In proportion as these changes are slight, so is the disease. 3. The fibrine is in general not diminished, nay, even sometimes increased. 4. There were some cases where, the fibrine having been diminished, the blood showed symptoms of dissolution.

5. Probably the change of the blood precedes the appearance of the disease, but it cannot be considered as its cause, although it increases the violence of the disease.

Becquerel and Rodier have confirmed by new experiments the fact that during pregnancy the blood globules diminish, and the fibrine and albumen slightly increase, but at the same time the specific gravity of the serum diminishes. This latter fact they imagine may give rise to many forms of dropsy, which arise towards the end of pregnancy; but, according to Vogel, the results are caused principally by mechanical disturbance of the circulation.

Scorbutus.—Becquerel and Rodier(*b*) conclude from their researches, 1st, that the old and generally received opinion that the blood in scorbutus undergoes a species of dissolution, is not correct, nor is the opinion of Majendie and of Andral and Gavarret, that the

(*a*) *Recherches sur la Composition du Sang dans les Fièvres Puerperales; Gaz. Med. de Paris*, No. 51, p. 991.

(*b*) *De la Composition du Sang dans le Scorbut; Gaz. Med. de Paris*, No. 26.

alkaline salts are increased, borne out; 2nd, the blood globules and soluble albumen were sensibly diminished, which Vogel accounts for by the loss of appetite; 3rd, the fibrine, contrary to the generally received opinion, was not diminished, but was even, in some cases, increased, and did not differ in its properties from that of healthy blood; 4th, the only positive change observed was a very considerable diminution of the specific gravity, which is much less than it ought to be from the quantity of solid matter contained in the blood. This curious fact cannot, as yet, be accounted for.

Dr. Neligan has communicated, in his paper on sea scurvy(*a*), two analyses of the blood, made by Dr. Aldridge, in this disease, which had reference principally to the inorganic constituents. The most remarkable difference is observed, in these analyses, in the total amount of inorganic matter, which, in one of the cases, was nearly double the quantity contained in normal blood. There was a marked difference between the amount of phosphoric acid in the two cases, although that which contained the least was still by no means below the normal standard. The potash and sulphuric acid were abundant, notwithstanding, as Aldridge remarks, that the food to the use of which scorbutus is usually attributed is deficient in them. The total amount of solid matter contained in both specimens was remarkable, and especially in one, which contained in 1000 parts only 684 of water, and 316 of solid matter!! It is much to be regretted that the proportion of fibrine, albumen, and blood-globules, was not ascertained in this most remarkable case. It is probable, however, that the blood-globules were in excess, which is not in accordance with the experiments of Becquerel and Rodier. Majendie, Andral, and Gavarret's opinion, that the alkaline salts are increased, is fully borne out, particularly in the case in which the large amount of solid matter was observed.

Carcinoma.—Heller(*b*) has arrived at the following conclusions from his investigations on this disease:—

1. The opinion of Engel, that this disease belongs to the class of *albuminous*, that is, to an increase of the albumen in the blood, is disproved by these investigations. For the blood in this disease, on the contrary, as in all others in which there is a tendency to new formations,—inflammation, rheumatism, tubercle (and probably also secondary syphilis),—is richer in fibrine.

2. In the higher grades, and in long-continued cases of cancer, the blood-globules decrease, and the water increases.

Heller also continually found in the blood of patients suffering from cancer, peculiar shining, golden scales under the microscope, which, of course, he was unable, from want of material, to determine the nature of, but which Vogel considers to be cholestearine.

Mania.—Heinrich's(*c*) results may be stated as follows:—In all

(*a*) Dublin Quarterly Journal, No. viii. November, 1847.

(*b*) *Pathologische Chemie der Krebskrankheit;* Heller's *Archives für Chemie und Mikroskopie,* Heft. 1. p. 28.

(*c*) Heller's *Archives,* Heft. 5.

investigated cases of recent mania, the blood did not exhibit any great variation from the normal composition. The blood of a patient labouring under this disease corresponds to that in a mild case of hydraemia. Mania, therefore, causes no peculiar condition of the blood, but is rather dependent on the existing condition of the body. In the seven cases examined, the fibrine was not increased, and consequently there was no acute inflammation.

Diseases of the Spine.—Becquerel and Rodier have analysed the blood of twelve patients labouring under diseases of the spine, with the following results:—

When the spinal disease is accompanied by paraplegia, the blood globules will be generally found to be diminished, and that in proportion as the patient is debilitated. The fibrine is at one time normal, at another increased; this latter sometimes from intervening inflammation, but also without. The serum has, in general, a high specific gravity, and is always rich in solid constituents, as well in albumen as in extractive matter and fat.

Bright's Disease.—Becquerel and Rodier have published a great number of analyses of blood in this disease, which confirm their former results, namely, that in this disease, in general, the globules diminish whilst the fibrine remains normal, except in inflammatory complications, where it appears to be increased, whilst the albumen is considerably diminished.

Pasquale la Cava(*a*) obtained 0·684 parts of pure urea from 1000 parts of the blood of a patient labouring under this disease.

Diabetes mellitus.—A great number of memoirs have appeared on this disease, of which we can only notice a very few, from our want of space.

It is well known that starch may be converted into dextrine and sugar by the influence of certain animal secretions, such as the saliva, pancreatic juice, &c. Majendie(*b*) has found that a great number of other animal substances possessed the same property, and, among others, the blood, not only in the body but outside it. This fact he has ascertained by a number of beautiful experiments, which want of space alone prevents our giving here. The natural consequence of these results is, that we must naturally seek for the cause of diabetes in the blood or in the process by which the blood is formed, that is, in the act of digestion; and on this basis two theories have been formed which have gained considerable reputation, the one by Bouchardat and the other by Mialhe.

Bouchardat's(*c*) theory is as follows:—The sugar of diabetes is principally, if not entirely, formed from starch and bodies belonging to the same chemical series. Diabetic patients digest this starch differently from healthy persons. Their stomachs contain a consi-

(*a*) *Annali di Chimica applicata alla Medicina*, Aprili, 1846, p. 242.

(*b*) *Note sur la Présence normale du Sucre dans le Sang., Gaz. Med.*, No. 38, p. 734.

(*c*) *Journal des Connais. Med. Chirurg.*, No. 4, April, 1816.

derable quantity of a nitrogenous body in a state of transformation, resembling that which vegetable albumen, &c., undergoes when it forms diastase. Indeed Bouchardat thinks from his experiment that this substance is diastase. By this, and by the large quantity of water which they drink, owing to continued thirst, the starch is rapidly converted into grape sugar, which is as rapidly taken into the blood. A quantity of grape sugar thus gets into the blood, which the lungs cannot burn off, and the excess must of course pass off by the kidneys. By this rapid solution and absorption of the food, and its expulsion again, unchanged, through the kidneys, fresh food will be required, hence the continual hunger of diabetic patients. But as the food consumed cannot be employed as nutrition, the patient wastes away, and his organs of digestion suffer from the excessive and vain employment of their functions. The case is different with healthy persons. The formation of sugar commences with them also in the stomach, but slowly, and is only completed in the intestinal canal; a very natural consequence, which Bouchardat and Sandras proposed in 1842.

Mialhe's(*a*) theory may be briefly stated as follows:—The starch of the food is converted into grape sugar, not only by diabetic persons, but also by healthy individuals, but in the latter cases it will be decomposed or burned off by the presence of alkalies, and under the influence of oxygen. This decomposition does not take place in diabetes, owing to the deficiency of alkalies in the blood, and the sugar, as such, is passed off by the kidneys. And this deficiency of alkalies is, according to him, to be accounted for by the suppression of the secretion of acid perspiration which always occurs with diabetic patients, and from the excessive consumption of acids which may be sometimes observed in this disease. His mode of treating the disease is, of course, founded on this view, namely, the administration of alkalies.

Costes(*b*) is of opinion that the disease cannot be accounted for by a purely chemical theory, as he found that it had been cured in a great variety of ways, some of which were incompatible with the theories given above, and from many other facts, particularly with regard to its cause.

Scharlau(*c*) is of opinion that diabetes is a disease which first originates in the spinal marrow, an abnormal vegetative function of the nervus sympathicus exhibiting itself in an abnormal process of digestion, and the consequence of which is that the nervus vagus is sympathetically affected, and an abnormal condition of the liver induced, which prevents the sugar from being converted into bile, and the excess is consequently carried off by the kidneys ; he is of

(*a*) *Traitement Raisonné par les Substances Alcaline ; Journal de Med. et de Chirurg, pratiques ;* Janvier et Fevrier, 1846.

(*b*) *Quelques Reflexions sur le diabete Sucré ; Jour. de Méd. de Bourdeaux,* Juillet, Aout, et September, 1846.

(*c*) *Die Zukerharnruhr nach eigenen Untersuchungen,* Berlin, pp. 100. 8vo.

opinion, therefore, that all attempts at cure must have reference solely to the spine.

The opinion of Scharlau has only the advantage of directing the attention of pathologists to the disturbance of the secretion of the bile; but it has also the disadvantage of resting on a mere supposition, namely, that the sugar is converted into bile, of which we have no proof whatever, a disadvantage which the theory of Bouchardat and Sandras avoids. They suppose that the blood can only burn off a certain quantity of sugar in a given time. If too much sugar comes into the blood from the stomach and intestinal canal, the excess will always be separated by the liver, and will come with the bile into the intestinal canal, and thence again into the blood. The liver is thus a sort of regulator of the metamorphosis of sugar in the circulation. This theory thus accounts for the fact that a deficient secretion of bile produces a greater amount of sugar in the blood, and an increase in its saline constituents, and consequently the production of diabetes. Bouchardat has also made animals diabetic for a time by causing them to consume a very large amount of sugary food.

But Bouchardat's theory does not explain how the grape sugar is decomposed in the blood of healthy persons. Mialhe endeavours to explain the question by the presence of alkalies. These alkalies are deficient in the blood of diabetic patients, and therefore the sugar is passed off undecomposed. But still this is an hypothesis, but it is one deserving of being tested by accurate chemical experiments.

Vogel makes the judicious remark, however, that if the theory of Mialhe be correct, why are the alkalies deficient in the blood of diabetic patients ? The explanation of Mialhe appears to him untenable, that it arises from the suppression of acid perspiration, because the quantity of acids thus given off is so extremely small, that a slightly increased acidity in the large quantity of urine passed would be more than sufficient to counterbalance it; and, as is well known in the commencement of the disease, the perspiration is often normal; and we may also remark, that there are a great many diseases where the perspiration ceases to be acid, and is often partially suppressed, and yet the sugar in the urine is not increased. Vogel further states that probably the opinion of Scharlau, that the disease arises from the increased thirst, may explain why, from the increased secretion of urine, and the consequent frequency of drinks, a greater excretion of salts, and especially of alkalies, may take place. This explanation, of course, remains to be proved by accurate experiments. But still Scharlau's opinion does not explain why there is increased thirst, for that which he has given is pure nonsense clothed in the metaphysical jargon of the anti-chemical school of physiologists, where a new name hides ignorance. Would that physiologists would give up metaphysics, and endeavour to become more *positive* and less verbose !

L. Lehmann(*a*) gives an account of a case of diabetes, attended with

(*a*) *Dissertation, &c., Erlangen,* 22 pages.

tuberculosis pulmonum, which was temporarily relieved by the use of animal food and cod liver oil. He detected sugar in the sweat, but not in the saliva ; the blood also contained it. In one analysis the blood globules were deficient, and the solid constituents of the serum very large. The urine in this case contained neither urea, uric acid, nor hippuric acid. The patient perspired profusely, which shows that the suppression of the perspiration is not a general cause of the disease. In corroboration of this opinion, I may mention that Aldridge long ago stated that sugar was a normal constituent of the urine; and that he in many cases detected it in the sweat, mucus of the nose, &c.

Flesh, and the Substances derived from it.—Liebig(*a*) has published a beautiful memoir on the juices of flesh, which from its importance will mark an epoch in physiological chemistry.

Chevreul discovered some years ago in meat broth a crystalline substance to which he gave the name of creatine, but from want of material, was unable to examine its properties very minutely, or to determine its composition. This Liebig has completely done. He obtained it in considerable quantity from the flesh of various animals, but most abundantly from the muscles of the heart of oxen. Creatine is soluble in water, from which it may be obtained in fine, shining needles, grouped like sugar of lead; in cold alcohol it is almost insoluble. It is a neutral body which dissolves in weak alkaline or acid liquids, without experiencing any alteration, but its properties are changed in the presence of concentrated acids, or of caustic alkalies. Strong acids convert it into an organic base, possessing some very remarkable properties, to which the name of creatinine has been given. Creatinine is formed in the presence of hydrochloric and sulphuric acids by the mere elimination of four atoms of water. It is more soluble in water and alcohol than creatine; its aqueous solution is caustic to the taste, like dilute ammonia, which it expels from its combinations. It turns reddened litmus paper blue, and forms, with all acids, salts, some of which are remarkable for their beauty, particularly the compound with platinum, which forms very fine crystals of a golden colour. Creatine may be represented by the formula $C_8N_3H_7O_4+2$ Aq., which contains the elements of one atom of glycocoll or sugar of gelatine, and one atom of ammonia. Creatinine is represented by the formula $C_8N_3H_7O_2$, which is equivalent to the elements of one atom of caffeine and one atom of amidogene.

Creatine and creatinine are constant constituents of human urine. The nitrogenous substance which combines with chloride of zinc, discovered by Pettenkofer in urine, is in fact a mixture of both.

If creatine be boiled with ten times its weight of hydrate of barytes, ammonia will be given off, and a new base produced called sarkosine. Carbonic acid remains at the same time in combination with the barytes. The formula of this new base is $C_6NH_7O_4$, which,

(*a*) *Ueber die Bestandtheilen der Flüssigkeiten des Fleisches; Annal. der Chem. und Pharm.*, Bd. 62, Heft. 3.

extracted from that of creatine, leaves the formula for one atom of urea. In the reaction there is also a small quantity of a substance similar to urethan formed.

Liebig also proved that lactic acid was a constituent not only of the flesh of the herbivora but also of the carnivora, and he obtained at the same time a new nitrogenous acid, to which he has given the name of inosinic. It does not crystallize, and many of its properties resemble lactic acid, and its formula is $C_{10}N_2H_7O_{11}$.

In his examination of the inorganic constituents of the juice of flesh, he obtained some very singular results. The inorganic constituents of the juice consisted of the alkaline chlorides, phosphates of soda, of lime, and of magnesia. The sulphates were totally absent. The ash gave a strongly alkaline solution, which, with the exception of that of the hen, contained salts of phosporic acid combined with two, and with three atoms of alkali, which was shown by its reaction with nitrate of silver.

The relation between the potash and soda salts in the juice of flesh and in the blood is very remarkable. To an equivalent of potash the blood of an ox contains twelve to thirteen equivalents of soda. This relation is reversed in the aqueous extract of the flesh of the same animal. From this it follows that the blood-vessels in the flesh supply a larger quantity of potash salts than of soda ; that consequently the blood-vessels have a different amount of permeability for each part of the body. This is most remarkable in the case of the secretion of milk, in which potash salts predominate. The constant occurrence of phosphate of soda in the blood, and of phosphate of potash in the juices of the flesh cannot certainly be accidental, but, on the contrary, must have connexion with the different functions; and hence the necessity of giving common salt with the food of animals in inland countries, where soda is not present in the fodder.

The juice of flesh, as is well known, reacts acid. This property Berzelius attributed to the presence of lactic acid, without, however, having proved it by any direct analysis ; this, however, as we have mentioned above, Liebig has done. Now the quantity of phosporic acid present in the juices of flesh is sufficient to neutralize the whole of the alkalies existing in them, and if no organic acids were present the resulting salts would be neutral. The quantity of organic acids contained in them, on the other hand, is not sufficient to neutralize the whole of the alkalies found, and consequently if no phosphoric acid existed in them they would, in this case also, react alkaline. The two classes of acids, however, are more than sufficient to neutralize the alkalies, and consequently acid salts of both must result, and to these the acid reaction of the juices must be attributed. In proportion as one acid is increased will the quantity of the other set free be also increased. Hence Liebig concludes that if the stomach receives from the blood the acids which are contained in the juices of the flesh, the blood must, during digestion, become more alkaline, and that, in order to return to its normal condition, it must either receive a quantity of acid from the muscles, or the excess of

alkalies must be transferred to the muscles, or removed by the kidneys. Liebig further concludes that, if the urine of animals be acid before digestion, it must become neutral or slightly alkaline during that process.

As blood and lymph are alkaline, and are only separated from the acid juices of the flesh by animal membranes permeable to both fluids, a great source of electricity must arise; and Buff, at the suggestion of Liebig, has constructed a voltaic pile, composed of disks of felt saturated with blood, and of brain and flesh, which produced a strong deviation of the galvanometer needle, the direction of the current being from the blood to the flesh.

Lymph.—Geiger, apothecary in Stuttgart(*a*), had an opportunity of examining the lymph from the lymph vessels of the foot of a horse, under the direction of Schlossberger. It was collected from a small crack beneath the fetlock of the hind foot.

The drops of fluid, which flowed out with varying rapidity, were clear like water, and, singly, almost colourless; in greater quantity, of a slightly yellowish colour, of a disagreeable smell, and a slightly salt taste. The specific gravity at a mean temperature was 1017. Immediately after it had flowed out a colourless gelatinous coagulum formed in it, which, in the course of a few hours, adhered together and floated on the surface. The subnatant liquid was perfectly clear, slightly yellowish, and exhibited under the microscope but very few globules.

It gave no reaction with test papers. The serum scarely became clouded on its being heated, and even after being boiled for ten minutes only a few flakes of albumen were thrown down; but when a few drops of acetic acid were added to the boiling fluid, a considerable precipitate was obtained. This, however, did not take place on adding it to the cold liquid. An addition of rennet (calf's stomach) and milk sugar scarcely troubled the liquid. When the serum was evaporated, a film was formed which was renewed when removed. On incinerating the residue, an ash was obtained which strongly effervesced with acids, and contained carbonic, phosphoric, sulphuric, and hydrochloric acids, combined with potash, soda, lime, and a trace of oxide of iron.

The fresh lymph, treated with potash, even in the cold, showed the presence of ammonia. Carbonic acid could not be detected with accuracy by the addition of other acids to the fresh lymph. The following is the result of its analysis:

Water, ...	983·7
Fibrine, ...	0·4
Albumen, ...	6·2
Extractive matter, ...	2·7
Fixed salts, ...	7·0

Together with traces of fat and of salts of ammonia.

(*a*) *Analyse von Lymphe, Archiv. fur Phys. und Heilkunde*, Jahr. 55, pp. 391.

Schlossberger has added some observations to this paper, which tend to throw considerable light on the nature of this substance, about which so much is said, and so little in reality known.

He first shows the purity of the lymph by the statement of the veterinary surgeon, by the white placenta of fibrine, and by its behaviour under the microscope, by which he was able to distinguish globules of different sizes and form, the largest of which were smaller than blood globules, evidently granulated, with sharp edges, and often with a visible nucleus; some were round and some elliptical. The smallest of them, in which no nuclei could be discovered, perfectly resembled the isolated nuclei. Both varieties were perfectly insoluble in ether. Neither blood globules nor fat globules could be detected.

The time of coagulation varied very much; from four to eighteen minutes after the discharge of the fluid. At first the placenta had a jelly-like transparency; afterwards it contracted considerably, and became opaque and more consistent; it never assumed a red tinge in the air. Schlossberger also mentions, as remarkable, the behaviour of the albumen, on heating the liquid, which was perfectly neutral. Although *free*, alkalies are not necessary to impart to albumen many of the properties of caseine, for it exhibits these characters when simply combined with soda; still it is probable that the presence of ammoniacal salts, which often react acid, had masked the alkaline reaction of the albuminate of soda. The lymph serum was not coagulated by ether.

In some experiments which Schlossberger made with the horse, by adding several salts to its drink, and, among others, ferrocyanide of potassium, he only obtained a negative result, even after two hours.

The quantity of lymph which flowed from the horse daily amounted to from three to five pounds. It suffered from this for several years, but did not appear to waste, although it lost strength.

Feathers.—Von Gorup Besanez(*a*) has found in the feathers of birds a considerable amount of silica, but no sulphates or chlorides. Henneberg has found silicate of soda or potash in the blood of fowls, a fact which explains the presence of silica in the feathers.

Gelatine.—Schlieper(*b*) has repeated the experiments of Persoz, Marchand, and myself, on the products of the oxidation of glue with chromic acid, and in general confirmed the previous results.

He obtained the following products:—prussic, acetic, valarianic, and benzoic acids, and further, two oily products, to which he has given the names of valeronitrile and valeracetonitrile, the former of which is decomposed into valerianic acid and ammonia, and the latter into acetic acid, valerianic acid, and ammonia. I consider, from

(*a*) *Erdmann und Marchand's Journal*, Bd. xxxix. p. 244.

(*b*) *Ueber die Zersetzungs Producte des Leims durch Chromsäure; Liebig's Annal.*, Bd. lix., p. 3.

my experiments, that both prussic and benzoic acids are only secondary products, and consequently not of constant occurrence.

Hippuric Acid.—Dessaignes(*a*) has made the beautiful discovery that hippuric acid is decomposed into benzoic acid and sugar of gelatine, by boiling it for half an hour with hydrochloric acid. This is a very simple decomposition, nothing else being produced, for if we subtract from one atom of hippuric acid, $C_{18}H_9N_2O_6$, one atom of benzoic acid, $C_{14}H_6O_4$, there will remain $C_4H_3N_2O_2$, which with $1\frac{1}{2}$ atoms of water is equal to half an atom of glycocoll, or sugar of gelatine. Dessaignes also found that nitric, dilute sulphuric, and even oxalic acids produce the same effects, and that these acids remain in combination with the sugar of gelatine, giving rise to a class of salts which have the property of acting as acids, and forming with salts of metallic oxides double salts, which are perfectly analogous to those formed with urea, described by Werther.

Mulder(*b*) was led from these experiments to repeat his examination of the sugar of gelatine, and has found that the composition formerly assigned by himself, Marchand, and Boussingault, was not perfectly correct, which he attributes to some impurities. The formula which he now proposes is $C_8H_{10}N_2O_8$, or, in the anhydrous state, $C_8H_8N_2O_6$. Laurent has verified Mulder's results, but he assumes a formula one-half that of Mulder's, namely, $C_4H_5NO_4$.

Horsford(*c*) assumes from a great number of experiments the same formula for glycocoll as Laurent. He has analysed a great number of the compounds which it forms as a base and as an acid, and the double compounds which it forms with salts. He starts the question whether glycocoll and benzoic acid may not as such form parts of the animal tissues, of albumen, fibrine, &c. In support of such an opinion he brings forward the facts that gelatine, by the action of sulphuric acid or potash, yields benzoic acid and glycocoll, and that when it is treated with chromic acid, it also yields benzoic acid. But he could not detect it in the urine, even after he had taken four grammes of it. He found, however, in the latter case, that the quantity of urea and uric acid was considerably increased. This curious effect he promises to pursue further.

Saliva.—Pettenkofer(*d*) has proved beyond all doubt the presence of sulphocyanogen in the saliva.

Cappezuoli(*e*) says that the sputa of persons labouring under pneumonia is capable of converting starch into sugar.

Wright(*f*), who has contributed so much to this branch of phy-

(*a*) *Nouvelles Recherches sur l'Acide Hippurique l'Acide Benzoique et le Sucre de Gelatine; Annal. de Chim. et de Phys.*, 3me serie, t. xvii. p. 50.

(*b*) *Untersuchung über den Leimzuker. Erdm. u. Marchand's Journ.*, Bd. xxxviii. p. 294.

(*c*) *Ueber Glycocoll und einige seine Zersetzungs Producte; Annal. der Chem.*, Bd. lx. p. 1.

(*d*) *Ueber den Schwefelcyangehalt des Menschlichen Speichels;* Buchner's *Repertorium.*, Bd. xli. p. 289.

(*e*) *Gaz. Toscana*, No. 21, 1845. (*f*) Chem. Gaz., No. 88, p. 240.

siological and pathological chemistry, gives the following as the ordinary constituents of saliva:

Water,	988·10
Ptyaline,	1·80
Fatty acid,	0·50
Chlorides of sodium and potassium,	1·40
Albumen, with soda,	0·90
Phosphate of lime,	0·60
Albuminate of soda,	0·80
Lactate of potash and soda,	0·70
Sulphocyanide of potassium,	0·90
Soda,	0·50
Mucus, with ptyaline,	2·60

Mialhe(*a*) has investigated the nature of the active principle of the saliva, which he considers to be analogous to diastase, and which can be isolated in the same manner as it. According to him it is prepared by filtering the saliva, and treating it with five or six times its weight of absolute alcohol. Being insoluble in this liquid, it is precipitated in white flakes. These are collected on a filter, and placed whilst moist in thin layers upon a plate of glass, dried in a current of air heated from 104° to 122° F., and preserved in a well-closed bottle. Its proportion in human saliva rarely exceeds $\frac{8}{1000}$ths. It is a white or whitish-grey, amorphous solid, insoluble in alcohol, but soluble in water and dilute alcohol. Its aqueous solution is neutral, and is not precipitated by the diacetate of lead. When set aside it changes and becomes acid. The acid which it produced is either butyric or one very similar to it. This principle has no action upon albumen, &c., nor on cane sugar, gum arabic, or woody fibre, &c.; but it has a remarkable action on starch, which it readily converts into dextrine and glucose. It contains, like diastase, nitrogen.

Cl. Bernard(*b*) has undertaken a new series of researches with reference to the statements made by former experimentalists, that an active ferment, similar to diastase, was only secreted by the submaxillary and sublingual glands, but that the secretion of the parotis did not possess that property; and has arrived at the following conclusions:

1. The mixed saliva, not only of the dog but of men and of horses, is capable of converting starch into dextrine and glucose, but does not possess this property at the moment of its escape from the glands. The secretion of the parotis is watery, that of the submaxillary adhesive and glutinous.

2. If a piece of the mucous membrane of the mouth be washed with water, and the liquid filtered, it will possess the fermenting property as well as the mucous membrane itself, when it has been

(*a*) *Compt. rend.*, April, 1846; and Chem. Gaz., No. 86, p. 196.
(*b*) *Archiv. Génér.*, Paris, Jan. 1847

exposed for some time to the air. The mucous membranes of the intestines, and of the bladder also, possess the same property.

From these experiments it is evident that there does not exist any peculiar ferment in saliva, but that the mucous membrane of the mouth which is covered and impregnated with substances in which, from exposure to the air, a peculiar state of metamorphosis is induced, supplies to the saliva particles of these bodies, and thus communicates to it its property of producing fermentation.

Digestion.—Bouchardat and Sandras(*a*) have communicated to the French Academy the following results of their researches on the digestion of spirits:

Spirituous liquors are only diluted, but not in any wise changed, in the stomach and intestinal canal, by the saliva, gastric juice, mucus, &c. It is absorbed through the veins, as Majendie already remarked. This principally takes place in the stomach, but when large quantities of it are consumed, or when it is mixed with sugar, it may also be absorbed in the intestinal canal.

The vessels which carry off the chyle absorb none of it. If fatty substances have been consumed at the same time, they may be found in considerable quantities in the chyle, but no alcohol.

When the spirits have passed into the circulation only a very small portion evaporates from the lungs, otherwise it is not separated by any organ of secretion. If too much alcohol passes into the blood, the arterial portion of it assumes the colour of the venous, and all the appearances of asphyxia may ensue. The alcohol can be converted directly into carbonic acid and water by the inspired oxygen, but in many experiments they found as an intermediate product acetic acid. Alcohol and its products disappear rapidly from the system. When taken with sugar or dextrine it disappears more rapidly than both.

Boussingault(*b*), in a beautiful series of experiments which he made on the assimilation of food, and which our space forbids us from further noticing, has singularly enough come to the same conclusion as Liebig did some years ago, and in this respect Dumas has been gradually following his example.

Carl Schmidt(*c*) has started a new view of the nature of the gastric juice. He denies the existence of a free acid in it as it is secreted by the glands, but assumes that a copulated compound of an organic substance (pepsine) with hydrochloric acid, analogous to sulpho-lignic acid, is afterwards formed He calls this active principle hydrochloropepsic acid. It is precipitated by salts of lead and mercury, and may be separated from them completely by sulphuretted hydrogen. Its acid solution possesses the power of dissolving animal substances in the highest degree. This complex acid is decomposed by

(*a*) *Sur la Digestion*, &c., *Compt. Rend.* t. xxiii. No. 2.
(*b*) *Experiences sur la Digestion; Annal. de Chim. et de Phys.*
(*c*) *Ueber das Wesen des Verdaungs Processes; Annal. der Chem.*, Bd. lxi. p. 311.

concentrated acids and alkalies, and the latter separates the pepsine from the hydrochloric acid in such a way that the addition of more acid does not reproduce the original compound.

Fat.—Redtenbacher(*a*) has shown, in a very excellent memoir, that by the action of nitric acid on oleic acid, that is by oxidation, not only are the volatile acids of butter, the caprylic, capronic, caprinic, and butyric acids, produced, but also acetic, metacetonic, valerianic, œnanthylic, and pelargonic (a volatile acid found by Pless in the Pelargonium roseum) acids. All these acids consist of a carbohydrogen, combined with O_4. From these experiments the occurrence of the above-mentioned acids in almost all natural fats, and particularly in human fat, is accounted for. Lerch has, for instance, found caprylic acid in considerable quantity in human fat. According to Redtenbacher, this acid has, in dilute solutions of it, precisely the same smell as that observed from healthy human perspiration; and he therefore thinks that after great exercise, or when the body is very much heated, this acid is volatilized.

The Bile and its Products.—Redtenbacher(*b*) has made the interesting discovery that cholestearine and choloidic acid, when treated with nitric acid, yield the same products of decomposition, which he calls cholesteric acid. It thus appears that cholestearine is allied to the non-nitrogenous constituents of the bile in constitution, and not to the fats. Acetic, caprinic, caprylic, and oxalic acids, &c., were also obtained as secondary products of this reaction on both substances.

Schlieper(*c*) examined cholic acid in the same manner as Redtenbacher has done the choloidic, and also obtained the cholesteric acid, but did not detect any of the other volatile products which Redtenbacher found, nor oxalic acid.

Schwendler and Meissner(*d*) have added to our knowledge of the composition of cholestearine. They likewise examined the products of distillation, but do not appear to have obtained any important results. They conceive that the hitherto received formula must be quadrupled, which will make it $C_{84}H_{72}O_3$ and the crystallized $C_{84}H_{72}O_3+2HO$.

Redtenbacher(*e*) has made the important discovery that taurine, one of the most remarkable products of decomposition of bile, contains 25·70 per cent of sulphur, which had hitherto been entirely overlooked, and that consequently the bile itself must be rich in that substance. He assigns to taurine the formula $C_4H_7NO_6S_2$.

(*a*) *Uber eine algemeine Enstehungsweise der Säuren* $(CH)O_4$ *Mit dem Siedpunct unter* 300° C.; *Annal. der Chem.*, Bd. lix. p. 41.

(*b*) *Ueber die Einwirkung der Saltpetersäure auf Choloidinsäure und Cholestearin; Annal. der Chem.*, Bd. lvii. p. 145.

(*c*) *Ueber die Einwirkung der Saltpetersäure auf Cholsäure; Annal. der Chem.*, Bd. lviii. p. 37.

(*d*) *Beiträge zur Kentniss des Cholestearins; Annal. der Chem.*, Bd. lix. p. 107.

(*e*) *Ueber die Zusammensezung des Taurins; Annal. der Chem.*, Bd. lvii. p. 170.

Verdeil(*a*) has succeeded in obtaining the bilate of soda, or principal constituent of the bile crystallized, and has thus confirmed Plattner's original observation. He gives as the formula of pure bilate of soda, $C_{44}H_{40}NSO_9+NaO$.

Von Gorup Besanez(*b*) has published a most excellent memoir on the bile, but of which we can only give a very short notice here. He shows that bile, by its spontaneous decomposition, yields the same products as when treated with dilute acids, namely, choloidic acid, taurine, and ammonia, the first being in greatest quantity. In one case he obtained cholic acid, a product which is obtained by the action of potash on bile. He also considers human bile to differ very little, if at all, from ox bile, contrary to the opinion of Kemp(*c*), who found on analysis that human bile contained four per cent. more carbon than ox bile, and also *more nitrogen*.

With respect to the colouring matter of the bile, Von Gorup considers that the bilifulvine, biliverdine, &c., of Berzelius, are only modifications of the same substance, and this again only a peculiarly altered blood-colouring matter, an opinion already expressed by Scherer, and after him by Polli. Von Gorup thinks the iron in the bile favours this view. He not only confirmed Bertozzi's original observation, that copper occurs in gall-stones, but he also shows its existence in the bile itself, though probably not as a constant constituent. He likewise confirms an opinion, by no means new, that the bile, at least the bilate of soda, has an antiseptic action on nitrogenous food and does not excite fermentation. He, like Pettenkofer, could not detect bilate of soda in the fæces although he found mucus and the colouring matter, and therefore concludes that it is reabsorbed.

He also touches on the connexion between the bile and the production of fat, the similarity of the choloidic and cholic acids, in their proportion of hydrogen and carbon, to fat, and on an observation of his on the presence of choloidic acid in the bile of pigs, which of all animals have the greatest tendency to produce fat.

Gundelach and Strecker(*d*) in their very complete memoir on the bile of the pig, which is distinguished from other biles by its being precipitated by acetic acid, deny that choloidic acid exists in it, as stated by Gorup-Besanez. The acid which he supposed to be choloidic acid they consider to be a peculiar acid, free from sulphur, to which they have given the name of hyocholeic acid, and that this acid replaces the bilic acid. Its formula, in combination with soda, is $C_{54}H_{43}NO_{10}+NaO$.

This acid differs from the acid of ox-bile in not being soluble in

(*a*) *Uber die krystallisirte Galle ; Annal. der Chem.*, Bd. lix. p. 311.
(*b*) *Untersuchungen über Galle ; Erlangen ;* also *Annal. der Chem.*, Bd. lix. p. 120.
(*c*) Trans. of the Cambr. Phil. Soc., vol. viii. p. 1.
(*d*) *Untersuchung der Schweine Galle ; Annal. der Chem.*, Bd. lxii. p. 205.

water, yields insoluble precipitates with lime, baryta, &c. As it does not contain any sulphur, it cannot yield taurine.

When the bile of the pig comes into contact with the chyme, which is always acid, hyocholeic acid must necessarily be precipitated and the acid of the chyme neutralized. This fact would render an examination of the process of digestion in the pig highly interesting.

Mulder(*a*), in a memoir which he has published on the constitution of the bile, endeavours to support the views of Berzelius on the nature of that fluid, and, of course, totally denies the existence of Liebig's bilic acid. He has, however, brought forward nothing particularly new.

The Bile in Disease.—Von Gorup Besanez has published in the memoir already quoted a great number of analyses of bile from patients labouring under a great variety of diseases, from which he draws the following conclusions:

1. The quantity of the bile varies very much, and, except in the case of typhus, where it is *generally small*, there appears to be no relation between it and the disease.

2. The colour is also very variable; the more concentrated the darker; generally very *clear* and of a light yellow in typhus, and in inflammation, especially of the organs of respiration; on the other hand it is dark, often black, in chronic abdominal diseases.

3. It has very little consistency in typhus and in inflammation; it is thick and tough, on the other hand, in chronic diseases.

4. The reaction is mostly neutral, seldom slightly alkaline, but it is sometimes acid. The latter results either from the decomposition of the bile or from pus, and the consequent formation of lactic acid. In typhus the bile is remarkable, like all secretions, for the rapidity with which it decomposes.

5. The water of the bile is increased, and consequently the solid constituents diminished, in inflammations, especially in pneumonia and typhus. From this, as well as from the general deficiency of colouring matter, it would appear that the constituents of the bile are retained in the blood, which has been actually proved in the case of pneumonia. Further, it deserves to be remarked that in typhus and pneumonia the quantity of the bilate of soda in proportion to the other constituents is diminished. In 100 parts of bile extract, for instance, from a case of pneumonia, there was 44·05 of bilate of soda, and in a case of typhus only 9·6, the normal proportion being from 60 to 70. The supposition, therefore, that there is a bilous pneumonia, would appear to be well founded.

6. The quantity of solid matter is increased in abdominal affections of the heart, &c. They also appear, contrary to the statements of Frerichs, to be increased in tuberculosis. The latter found them so, and they were only increased when some affection of the liver was present at the same time.

(*a*) *Untersuchungen über Galle, &c.*

7. The quantity of mucus is in general in the inverse ratio to that of the other solid constituents, and generally varies between one to six per cent. In typhus it appears relatively increased from the deficiency of the other solid constituents. In fact, in this disease, the gall-bladder often contains nothing but mucus.

8. The fat appears to be generally increased in colliquative diseases, typhus, and tuberculosis, even where the microscope shows the presence of fat globules and crystals of margaric acid. Crystals of cholestearine are in general rare, and mostly occur where the bile is very concentrated in abdominal complaints.

The Urine and its Constituents.—*Combination of Urea with Salts.*—Werther(*a*) has examined this subject very carefully, and has obtained some very beautiful results. The combinations of urea with salts are merely held together by a very weak affinity, and appear to exist only with salts whose solubility in water or alcohol does not differ much from that of the urea. Most of the salts formed are not decomposed by boiling, and some not even by nitric and oxalic acid.

Urea forms with nitrate of silver two compounds, represented by the formulæ $Ao_5O_5NO_5 + C_2H_4N_2O_2$ and $2(Ao_5\,O, NO_5) + C_2H_4N_2O_2$. The first compound crystallizes in large, shining, rhombic prisms with oblique surfaces, which are soluble in cold and hot water and alcohol. When the aqueous solution is boiled for a long time, it is partially decomposed, and cyanate of silver formed; nitrate of ammonia also appears to have been produced. The second salt forms large, shining, rhombic prisms, with plain terminal surfaces. A number of other compounds with nitrates were obtained, many of which exploded when heated.

It unites with chloride of sodium, forming a salt which crystallizes in very shining rhombic prisms, with oblique terminal surfaces, which deliquesce in the air. They are very readily soluble in water, are decomposed in boiling or cold absolute alcohol, a small quantity of NaCl being dissolved with the whole of the urea. But if a tolerably concentrated aqueous solution of the crystals is mixed with ten to twelve volumes of alcohol, nothing separates, even after a considerable length of time; indeed, a certain excess of nitric acid then no longer produces any precipitation. This circumstance is very important in the determination of urea in urine, as this constantly contains chloride of sodium. The formula of this compound is $NaCl + C_2H_4N_2O_2 + 3HO$.

He did not succeed in getting crystalline compounds with the chlorides of potassium, ammonium, or barium.

From the composition of the above compounds, urea may be regarded both as CH_2 and as $C_2H_4N_2O_2$; however, the two compounds with nitrate of silver speak very much in favour of the composition $C_2H_4N_2O_2$; as otherwise, if the second silver salt were considered

(*a*) *Erdmann und Marchand's Jour.*, Bd. xxxv. p. 51; and Chem. Gaz., No. 79, p. 58.

as $AgONO_5 + CH_2NO$, the first would become $2(AgO_1NO_5) + 2(CH_2NO)$, and the lime and magnesia compounds would have a still more complicated composition.

On the Action of Chlorine on Urea.—De Vry(*a*) observed that, by heating in an oil bath muriate of urea, obtained by passing dry muriatic acid over powdered urea to a temperature of 293° cyanuric acid was obtained, chloride of ammonium being at the same time formed. When, instead of removing the bath at 293° the temperature is raised to about 608° and the mass treated with water, the white body discovered by Liebig and Wöhler was obtained instead of cyanuric acid. Wurz(*b*) obtained cyanuric acid by treating the fused urea with dry chlorine instead of with hydrochloric acid. In addition to the products above mentioned, hydrochloric acid and nitrogen were also given off.

On a new Product of Decomposition of Urea.—Wiedemann(*c*) found on heating nitrate of urea, as also pure urea, a new body, for which he proposes the name of *biuret*. It is easily soluble in water and alcohol, crystallizes from the former with two equivalents of water, which it parts with at 212°, and from alcohol anhydrous. Its composition is represented by the formula $C_4H_5N_3O_4$. Biuret dissolves unaltered in sulphuric acid, and is destroyed only by long continued boiling in it; it is not precipitated from its solutions by bases, acids, or metallic salts; and is, as far as the author is able to judge from his experiments, a neutral substance. It is especially characterized from its solution acquiring an intensely red colour on the addition of caustic potash and solution of sulphate of copper. It fuses when heated, and is converted with loss of ammonia into cyanuric acid. He considers that when urea is heated it is first converted into biuret, and then into uric acid.

On some new Products of Decomposition of Uric Acid.—Schlieper(*d*) has examined some new products obtained from alloxanic acid, and also the salts of that acid, and has added very much to our knowledge of that class of bodies which the classic labours of Liebig and Wöhler first made known.

If an aqueous solution of alloxanic acid be boiled, or retained at a nearly boiling temperature, it is completely decomposed, carbonic acid being copiously evolved. If this solution be evaporated rapidly to the consistence of honey, and then treated with water, a portion only is dissolved, whilst a dazzling white crystalline powder remains behind. The dissolved portion, after having been again evaporated and treated with water, leaves another portion of the white powder undissolved. The alloxanic acid is thus decomposed into carbonic acid and two new substances; the one which is insoluble in water is a new acid, called leucoturic acid; the soluble substance is a neutral body, called difluane.

(*a*) *Annal. der Chem.*, Feb. 1847; and Chem. Gaz. No. 109, p. 182.
(*b*) *Compt. rend.*, March 15, 1847. (*c*) Chem. Gaz. No. 124, p. 482.
(*d*) *Annal. der Chem.*, October, 1845; and Chem. Gaz. No. 77, p. 1.

Leucoturic acid constitutes the smallest part of the products of the decomposition of alloxanic acid by heat. It forms a snow-white granular crystalline powder; it is insoluble in cold water; tolerably soluble in boiling water, requiring, however, some time. It is not acted upon by acids; it is readily soluble in alkalies, and precipitated by acids, provided the solution is recently prepared, and no heat has been applied. If it be kept for some time ammonia is evolved, and oxalic acid remains. Its formula is $C_6H_3N_2O_6=C_6H_2N_2O_5+HO$.

Difluane forms the largest portion of the products of decomposition of the alloxanic acid. When perfectly dried it forms a transparent, brittle, gummy, vesiculous mass, which is easily pulverized. It is very soluble in water; the solution does not crystallize, nor is it decomposed by boiling; it is slightly acid, of a bitter and saline taste, and yields white precipitates with salts of lead and silver. It does not combine with ammonia. It is decomposed by solution of potash, even when cold, ammonia being evolved, and oxalic acid being formed in the solution. When heated with nitric acid it is decomposed with effervescence, alloxan being formed, but no alloxanic or parabanic acid. Sulphuric acid appears to exert a remarkable decomposing influence, both on difluane and lencoturic acid. The formula is $C_6H_4N_2O_5$. The formation of these bodies from alloxanic acid may be accounted for thus:

1 atom of leucoturic acid, . . .	6C	3H	2N	6O
1 atom of difluane, ...	6	4	2	5
4 atoms of carbonic acid, . . .	4	0	0	8
1 atom of water, ...	0	1	0	1
2 atoms of alloxanic acid, . . .	16C	8H	4N	20O

This decomposition of alloxanic acid into leucoturic and carbonic acids, and difluane, is exactly similar to that of its isomeric compound alloxan, by boiling its aqueous solution, into alloxantine and parabanic and carbonic acids.

If a solution of alloxantine be rapidly boiled down to a small volume with excess of hydrochloric acid, it undergoes a partial decomposition. Another white pulverulent body separates, mixed with alloxantine, from which it can be freed by treatment with nitric acid, in which it is insoluble. This substance is a new acid, to which the name of allituric acid has been given. It is soluble in about from fifteen to twenty parts of boiling water, forming a yellowish fluid, from which the acid separates as a voluminous crystalline powder, of a somewhat yellowish colour on cooling. It is soluble in concentrated sulphuric acid, and is precipitated, unchanged, on dilution with water. When heated with nitric acid it undergoes no change. It dissolves in potash, but is completely decomposed by it, ammonia being evolved. Its formula is $C_6N_2H_2O_3+HO$.

From the solution of alloxantine filtered from the allituric acid, the ammonia salt of another acid was obtained in splendid, shining, yellow laminæ. The acid itself could not be separated, being retained in its compounds with remarkable force. The ammonia salt

is almost insoluble in cold water; more soluble in hot. It is perfectly insoluble in ammonia, and its crystals do not lose any of their lustre in it. It is perfectly insoluble in and undecomposable by nitric acid. Strong sulphuric acid dissolves it, and on dilution the acid is precipitated. It is soluble in dilute solution of potash, ammonia being evolved. When the dry salt is heated it burns readily, and glimmers like tinder. Its formula, considered as a bibasic acid, in combination with ammonia, is $C_8N_3HO_8,NH_4O + HO$. Schlieper regards it as a conjugate acid, in which three atoms of cyanic acid are combined with two of carbonic acid and water.

Extractive Matter.—Scherer(*a*) has published a very valuable contribution to our knowledge of one of the most difficult points, connected not only with the urine but with all the animal secretions, which, like all his other memoirs, is remarkable for its clearness, and for the importance of the results at which he has arrived. These results he sums up as follows:

1. The greatest part of what has been heretofore considered as extractive matter of the urine, is a peculiar colouring matter, which, by treatment with salts of lead, and with acids, may be decomposed into a number of parts having different constitutions.

2. This colouring matter is, in the normal condition of the system, poorer in carbon and hydrogen than that of the bile.

3. It is probable that both are derived from the hæmatine of the blood, an opinion also held by Polli.

4. As the colouring matter of the urine has not always the same composition, and from its behaviour under the influence of the air, it may be deduced that it, as well as the substance from which it is produced in the blood, is in a state of constant chemical metamorphosis, which may be considered as oxidation or eremacausis.

5. This oxidation takes place as well with the hydrogen as with the carbon, for the former also varies; and it thus affords a proof of the production of water in the process of combustion going on in the animal body.

6. This urinary colouring matter occurs in the urine in different conditions; one part is richer in hydrogen and carbon, and others poorer.

7. The richer the food is in carbon and hydrogen, the habits being the same, the richer appears to be the secreted colouring matter.

8. The more organic material is wasted within a given time by the processes of the system (as in fever), without the act of respiration, and the activity of the liver being at the same time proportionably raised, the less *intense* appears to be the process of decarbonization of the constituents of the blood, although it may be increased quantitatively.

9. Conditions similar to those which occur in the production of

(*a*) *Uber die Extractivestoffe des Harns; Annal. der Chem.*, Bd. lvii. p. 180.

urea and uric acid appear to exist in the production of these colouring matters. We find, for instance, with an increased metamorphosis, that is, an increased consumption of organic materials, that the uric acid, which is rich in carbon, is increased wherever there is not at the same time an increase of action in the functions of respiration, and in the activity of the liver; that is, when the *intensity* of the action does not keep pace with the *quantity*. This analogy is also evident from the fact that a urine rich in uric acid has in general a colouring matter rich in carbon and hydrogen.

10. Although, as a general rule, the carbon and hydrogen appear to diminish and increase in pretty nearly the same proportion, yet cases occur where the hydrogen is much more diminished than the carbon.

11. From an immense number of observations made on patients labouring under disease, it may be deduced that the richer the urine is in these colouring matters, the greater is the waste of blood globules in the vital functions. As a general rule, in long-continued anæmia and hydræmia such is the result.

Free Sulphur and Phosphorus in Urine.—Ronalds(*a*) supposes from the circumstance that urine when it putrefies gives off sulphuretted hydrogen, that there exist other compounds of sulphur in that fluid than sulphates. In some experiments which he made on this point he found the quantity of unoxidized sulphur passed in twenty-four hours to amount to from three to five grains. He also considers that phosphorus exists in the same condition and to the same amount. This statement has been doubted by Scherer, but Aldridge, in his excellent lectures on the urine had come to the same conclusion several years ago. Ronalds states that the quantity of unoxidized sulphur contained in diabetic urine often amounts to four times that contained in healthy.

Creatine in Urine.—Heintz(*b*) has proved that the body which he discovered in the urine almost simultaneously with Pettenkofer's discovery of the peculiar compound formed in urine with chloride of zinc, is identical with that of Pettenkofer, and is in fact creatine, a fact fully established by Liebig's beautiful researches.

Urea in the Liquor Amnii.—Wöhler(*c*) has detected the presence of urea in the liquor amnii of a woman.

Pathological Chemistry of the Urine.—*Leucine and Creatine in Urine.*—About a year since, I read a short paper before the Dublin Pathological Society, containing the results of an analysis of the urine of a patient in the Meath Hospital. He had been for a long period in India, and his constitution was completely broken up. He appeared to be principally affected in the liver and kidneys. His urine contained a large quantity of albu-

(*a*) Phil. Trans., 1846.

(*b*) *Ueber das Kreatine im Harn; Poggendorff's Annal.*, Bd. lxx. No. 13, p. 466.

(*c*) *Harnstoff im Fruchtwasser; Annal. der Chem.*, Bd. lviii.

men, and deposited a beautiful pink sediment, which had all the properties of murexide, or purpurate of ammonia. The liquid, filtered from the coagulated albumen, contained distinct traces of benzoic acid, abundance of lactic acid, leucine, scarcely a trace of urea, but, on the contrary, a very large quantity of creatine. I was able to obtain beautiful pure specimens of the two latter, and sufficient of the lactate of zinc to make several analyses. The presence of leucine is certainly very remarkable, and indicates that a process analogous to a perfect putrefaction was going on in the patient's system; this would also seem to be indicated by the presence of the lactic acid, which is also a product of the putrefaction of animal substances. I was unable to detect any sugar. Just as the case became interesting he died, and consequently I was unable to study the matter further. I believe, however, that leucine and sugar of gelatine are frequent constituents of urine, although not hitherto noticed. We know so little about creatine, that I can say nothing about its relation in the present case.

Oxalate of Lime in Urine.—Pickford(*a*) notices the occurrence of oxalate of lime in the urine, the various opinions relative to its importance, and the cases in which it has been as yet noticed. He observed it in the urine of a peasant aged 23, suffering from cardialgia accompanied by anæmia, in typhus, empyema, pleuro-pneumonia, pemphigus, typhlitis chronica. In all the cases anæmia was present.

Dr. Aldridge's experiments on this point do not appear to be as well known as they deserve. He has shown that uric acid by the addition of the element of water contains the elements of oxalic, carbonic, hydrocyanic, and formic acids, and ammonia; and that, under certain circumstances, it may be actually converted into those bodies. Thus he states if urine which exhibits no reaction for oxalic acid on being passed be kept for some time and then simply boiled, oxalate of lime will be deposited, and the existence of hydrocyanic and formic acid may be readily determined. These experiments of Aldridge point out the necessity of examining the urine in a fresh state, and without boiling, otherwise the detection of oxalate of lime would be of no importance, as it may, should these experiments be confirmed, be derived from the simple decomposition of uric acid after the secretion of the urine.

Variations in the alkaline and earthy Phosphates in Disease.—Bence Jones(*b*) has made a number of experiments to determine the total amount of the phosphates contained in the urine voided by patients labouring under encephalitis on the one hand, and under delirium tremens on the other. The average specific gravity of the urine in encephalitis was 1025·3, and the average quantity of phosphates in 1000 parts 8·26. The average specific gravity of the urine in delirium tremens was 1020·4, and the quantity of phosphates only 0·67 in

(*a*) *Notizen zur Untersuchung des Urins; Zeitschrift fur Rationelle Medecin von Henle und Pfeufer*, Bd. v. p. 45.

(*b*) Chemical Gazette, No. 87, p. 226.

1000 parts. The general results at which he has arrived are: 1. That acute affections of the nervous substance, organic and functional, are the only diseases in which an excess of phosphatic salts appears in the urine; and in acute inflammation of the brain its amount is proportional to the intensity of the inflammation. 2. That in a large class of functional diseases of the brain, of which delirium tremens presents the most marked example, the secretion of phosphates is most remarkably diminished. 3. That no chronic disease exhibits any marked excess in the total quantity of phosphatic salts secreted.

Urine in Insanity.—Erlenmayer(*a*) divides the urine of insane persons into two classes :—1. Urine of those suffering from melancholia, mania, madness, or paraphrenie. 2. Dementia or anoia, and idiopathic epilepsy.

The urine of the first class is pale, poor in solid matter, particularly in uric acid and its compounds, and the urea in mania has a great tendency to decomposition, and consequently to alkalescence. This tendency to decomposition is much less in melancholia, and in proportion as it approaches mania or paraphrenie the tendency increases. Such urine, on this account, readily deposits phosphatic sediments. Erlenmayer never, however, observed an absolute increase of the phosphates even in mania. He generally observed the urine to have a tendency to alkalescence in the state of depression which frequently precedes mania, and in the lucidis intervallis in periodic mania; and in fact it was often the only premonitory symptom. In insanity, but especially in mania, there were very frequent attacks of polyuria, which disappeared as the disease abated.

The urine in the second class was, on the other hand, strongly coloured, rich in solid matter, especially in uric acid and its compounds, and consequently of high specific gravity, and having very little tendency to alkalinity. The sediment occasionally observed in it contained uric acid and its compounds, which does not agree with the statements of Sutherland and Rigby.

The urine in periodic mania is perfectly similar to that in epilepsy, in which in general he observed after each attack crystals of uric acid.

The more solid constituents the urine contains, without at the same time an improvement in the condition of the mind taking place, the darker will be its colour, and the more abundant the deposit of uric acid and urates, and the more unfavourable the prognosis.

Erlenmayer gives as the maximum specific gravity of the urine in the morning 1032, while Sutherland and Rigby give 1040.

Of foreign substances fat was the only one frequently present, while albumen, sugar, pus, and the constituents of the bile, were very seldom observed.

Urocyanine.—Aloys. Martin(*b*) has observed, that on the addition

(*a Uber den Harn der Irren; Archiv. für Phys. Heilkunde*, 1846, p. 684.
(*b*) *Uber das Urokyanin und einige andere Farbestoffe im Menschen Harn. Pathol. Chem. Inaugural abhandlung.* München, 1846.

of muriatic acid to many urines an amorphous, more or less blue-coloured powder separates with the crystalline uric acid. This substance appears to be a mixture of various colouring matters, passing from amaranthus-red through dark Burgundy-red to indigo-blue, which, by the use of ether, and of cold and boiling alcohol, can be partially separated from one another and from uric acid. He found that the blue colouring matter, to which he gives the name of urocyanine, was always the principal constituent. It is very soluble in alcohol, contains nitrogen, and volatilizes like iodine, giving a similar coloured vapour. The pathological conditions in which Martin found the urocyanine were sometimes accompanied by an increased quantity of urea, and sometimes by a diminution of it. He also found that the more the lungs were disturbed in their deoxidizing function, and the liver in its decarbonizing function, the pigments became darker. Thus it was found in pneumonia, especially during the first days, in a dark-coloured, saturated urine; in acute rheumatism, as well of the joints as of the tendons and muscles; typhus in all the three stages; scarlatina during the scaling off of the epidermis; chlorosis, when gastric symptoms were present; anæmia, dropsy, tuberculosis, acute scirrhus, dyspepsia, &c.

MEDICAL MISCELLANY.

Postscript to Dr. Graves's *Paper on the Diffusion of Cholera, in the present Number.* *See pages* 1 *to* 39.

It gives me much pleasure to find that, since the preceding observations were written, suitable measures are at length being taken for the treatment of cholera patients in this city, under the direction of the Board of Health, and with the co-operation of the Boards of Guardians. Several hospitals will, I am told, be ready for the reception and treatment of poor patients attacked by the disease, and ample accommodation will be provided for those who have the misfortune to labour under the epidemic. The announcement of the fact that, when cholera arrives in the city (which I fear it will do at no very distant date), it will not find us altogether unprepared, will be hailed with pleasure by every humane person who has the interests of his fellow-citizens at heart. I feel bound to acknowledge that the public owe a deep debt of gratitude to the Sanitary Committee, and to its respected chairman, Sir Edward Borough, in particular, for availing themselves of every means which the law enabled them to employ in effecting this desirable result. I have no doubt likewise that the Board of Health have conscientiously discharged the practical duties imposed on them by the Legislature; I quite willingly accord to them all the praise they are entitled to on

that score. But I cannot retract a single word of what I have already said concerning the absurd position in which the Boards of Health, both in Dublin and in London, have placed themselves by their first "Circular Manifesto;" and in which they ventured to assert in an authoritative manner, that "all experience" proved that cholera was not contagious, and that there was no danger to persons coming in contact with individuals labouring under the disease; and, consequently, no objection to admitting cholera patients into general hospitals. The experience gained in the former epidemic I have already dwelt on; and I now wish to add the details of some facts observed during the present epidemic, which appear to be decisive of the question, and consequently fatal to all claims the Board of Health here can have to be guides, either of the public or the profession, on this vitally important question.

The first testimony to which I wish to direct the attention of my readers is taken from the statistics of the cholera hospital in Surgeon-square, Edinburgh, by William Robertson, M. D., a gentleman well known as an accurate observer and talented physician. The paper to which I advert is published in the January Number of the Monthly Journal, and Retrospect of the Medical Sciences. Dr. Robertson, at pp. 457-8, records the following facts:

"With every deference for the opinion held by many members of the profession, and promulgated by the London Board of Health, I feel constrained to admit the evidence in favour of the contagious properties of cholera, or, at least, without insisting upon the literal application of the term contagion, to believe that there is something in the presence of the sick likely to induce the same disease in the sound. If we grant the existence of a morbific poison emanating from the bodies of the sick, it seems natural to infer that it must be energetic in proportion to its degree of concentration, and that the clearest manifestation of its virulence may be looked for in hospitals. Since the 28th October eighteen nurses have been employed at different times in the cholera hospital; five of these have had severe attacks of cholera, and three have died. A sixth, who for twelve hours had frequent vomiting and purging, was treated as a cholera patient, and recovered. Yet, at the same time, a very large number of nurses were employed in the neighbouring buildings of the Royal Infirmary, and escaped the disease. The circumstances, habits, and diet of both sets of nurses are the same; the work is not more severe in the cholera hospital than in the Infirmary; the arrangements for ventilation and cleanliness are not defective in Surgeon-square; but for nearly two months past no cholera patients have been admitted into the Royal Infirmary. In illustrating the contagious properties of typhus fever, we are accustomed to regard evidence of this sort as tolerably conclusive.

"Other two cases have occurred to me, which seem interesting in connexion with this subject. About the 20th November, a young woman was suddenly seized with illness on the street; she com-

plained of pain in the abdomen, and was immediatly sent by some of the passers-by into the cholera hospital. On admission, there was no evidence of cholera, but the patient was in labour, and in an hour or two gave birth to a fœtus in the third month. After four days' residence in the hospital, the patient was, at her own request, dismissed. She was, a few days afterwards, re-admitted, with the unequivocal symptoms of the disease, and stated that the premonitory diarrhœa had manifested itself two days after her dismissal, while she was an inmate of the House of Refuge. The patient died after a few days' illness.

"The second case relates to the importation of the disease from an infected locality into one which had for some weeks been healthy. A woman who had premonitory symptoms of the disease while residing in an infected lodging in the Horse-wynd, removed to Currie's-close, in the Grass-market, and, after passing thirty-six hours in this new locality, was brought into the cholera hospital. It had been remarked as rather singular, that for several weeks before no case had been reported from Currie's-close, notoriously one of the filthiest closes in the old town of Edinburgh; and it was not unnatural to expect that, after this importation of the disease into a locality where the predisposing causes existed in such perfection, other cases would speedily show themselves. Within the next few days several did originate in Currie's-close; and had not a number of its miserable inhabitants been speedily removed to the quarantine establishment in Forrest-road, there is little doubt that many of them would have suffered."

The Board of Health will, if I am not mistaken, find in the above facts some materials for reflection, if not regret; for they cannot but feel that, as far as their authority is concerned, they have been instrumental in propagating the belief, that nurses may be employed in attending on cholera patients, without thereby incurring any danger of contracting the disease; an assertion which I have shown is totally at variance with the truth. During the visitation of such an epidemic, nurses must be employed, but let the risk which they thereby run be, as far as it is possible to do it, compensated by the increase of their pay. To this point I wish to draw the attention of the public, and of the Boards of Guardians generally throughout the country; and I think it right also to observe, that, with respect to the physicians who are destined to treat cholera patients, the remuneration to be allowed is not to be measured solely by the labour they must undergo, or the skill which they must exercise. It should be considered in reference also to the danger which they thereby necessarily incur. It is to be hoped that the authorities will exhibit more consideration for the profession than they did during the unfortunate fever epidemic of 1847. But to return to the position of the Board of Health, "that all experience proves that cholera is not contagious;"—let us see how it is borne out by the introduction of the disease into Belfast. On this subject I have received a letter,

dated the 6th January, from Dr. Henry M'Cormac, whose writings on fever and other subjects have earned for him a well-founded reputation:

"You are aware that cholera occurred here for the first time in the person of an individual (with his family) who arrived direct from Edinburgh. He died in the workhouse hospital. The subsequent cases, between twenty and thirty in all, be it more or less, were confined to the workhouse, partly among adults, partly children. The first two cases that ensued in town were, first, an individual who had been discharged from the workhouse. He shortly after, I know not in how many hours, exhibited symptoms of cholera, and died. Ten hours unfortunately elapsed without his receiving medical assistance. The second case was that of a girl who, with others, had been in to see this man. The gentleman who visited her told me that the floor was covered with choleric dejections. She died. The next town case came, I believe, direct from Glasgow, as was assured me by more than one individual who came in the same vessel, and as, indeed, was a matter of notoriety. This case was taken to the General Hospital. It ended fatally. Up to yesterday seventy-three cases, partly workhouse, partly town cases, have been reported in all. Of these, twenty-nine cases proved fatal, sixteen remaining over. The course of the disease here leads, in my mind, to no other conclusion, than its communication and communicability from person to person. Such was also the result of my experience when cholera prevailed in Belfast before."

Nothing surely can be clearer than this evidence, and no one, after reading the details of the preceding facts, can attempt to deny that the cholera was introduced into Belfast by importation from Edinburgh and Glasgow, and that its capability of being imported shows that it is contagious. And in a subsequent communication Dr. M'Cormac informs me, upon the authority of the apothecary to the hospital, that "of those attacked with cholera in Belfast about seventy were inmates of the work-house, and about two-thirds of these were children. It is worth observing, that, since the removal of the children from the nursery to other apartments in the house, there are much fewer of them the subjects of the disease. The porter, one of the nurses, and an assistant nurse, all of whom were much in the way of the disease, have been attacked. The two former are now convalescent and walking about: the last, I regret to say, died." Again, in the *Globe* of Saturday, January 6, I find it stated in a communication from Hertford: "The tramp at Crane's lodging here is recovering; but Mrs. Tyler, the workhouse nurse, who attended him, has since died from the disease, as has also a child living in the opposite house."

Since the first portion of this paper was written I have received a detail of some interesting facts that occurred in the former epidemic, from Dr. T. H. Baker of Parsonstown, which so evidently demonstrate the contagiousness of cholera, that I fear that even the Autocrats who compose the Dublin Board of Health will be unable,

by the greatest exertion of their ingenuity, to induce the public any longer to acquiesce in the *dictum* contained in their celebrated Ukase.

"The first case of cholera," writes Dr. Baker, "which appeared in the Lorrha district, where I had a dispensary, in 1832, was that of a woman who drove her own ass and car. Midway between Birr and Portumna (fourteen miles), she was taken so ill that she was obliged to ask a bed from a very poor family, for the night. However, she died of cholera in a few hours. The next day the four persons residing in the house were attacked, and *died, all within twelve hours after the disease showing itself amongst them.* From this period the disease spread rapidly in a very small district (within a circumference of one and a half miles), so that in five weeks there were seventy cases and thirty-four deaths: no case appearing anywhere else between Birr and Portumna during the whole year. Perhaps I should add that many of the neighbouring people attended the wake of the first woman.

"In the year 1834, Mrs. Carty, of Ross (fifteen miles from Birr), went to a watering-place in the county of Galway (fifty miles from Ross), where there were some occasional cases of cholera. Finding herself not very well, she determined on coming home, and started forthwith, making the journey in one day. On that night she died of cholera; next day her husband and one of her children were taken ill, and died on the following day. A married daughter, who resided twenty-five miles off (near Nenagh), hearing of her mother's illness, came for the purpose of seeing her, and found that she was dead, and her father and brother dying of cholera. She took alarm and started for home almost immediately. She, however, caught the disease and died. Her husband, who had not left home, took the disease two days after, and recovered: one of his children also took cholera, and died. There had not been a case of cholera between Galway and Ross (fifty miles), or between Nenagh and Galway (seventy-five miles), for the previous year. The disease was confined to the two houses. No person in the neighbourhood ventured near them. These cases are, in my mind, perfectly conclusive, not that I had the least doubt before of cholera being contagious."

The increase of cholera in Glasgow during the depth of winter proves that we cannot count upon cold as an agent capable of counteracting the spread of cholera; and this statement is confirmed by the last news received from St. Petersburgh, and from which we learn that the cholera which had prevailed in that city during the summer months, and had slumbered during September and October, had awakened with a greater degree of energy in the commencement of November, just as the severe cold of winter had set in, and that its ravages at that period far exceeded those it had committed during the heat of summer.

I have to call the attention of my readers to another official circular, issued by the Board of Health in London, and dated Gwydyr

House, November 11, 1848. The General Board of Health, in this document, do not scruple to reiterate the opinion that it is important the public should know that the entire body of physicians and surgeons who have had the largest experience of this disease, not only in India but also in every city in Europe, entertain a decided opinion of its non-contagious nature, with a few individual exceptions. Having received many communications from physicians in various parts of the United Kingdom, since the publication of the last Number of this Journal, I must beg leave altogether to deny the truth of this assertion; and I have no doubt that, were the opinions of the whole profession deliberately taken, it would be found that the weight of authority was greatly in favour of the disease being contagious(*a*).

There is another point brought forward by the London Board of Health which requires a passing notice. They state that it has been observed that, concurrently with the outbreak of almost all great epidemics, there is some atmospheric condition, which, in proportion to the power and extent of the epidemic, depresses the general health of the public; and evidence of this, is more definite and full in regard to cholera, than in reference to any other epidemic. Such a statement made by the Board of Health is most astounding; for, as I have before shown, at the very moment they were making it, a reference to the reports of the Registrar-General would have been sufficient to convince them that the general health of the population was by no means depressed, but on the contrary was unusually good in all the very districts where cholera had made its appearance, in England or Scotland.

In the face of such an undeniable fact, even Dr. Adair Crawford's curious observations concerning the extraordinary changes which, according to him, prevailed in the state of the atmosphere in St. Petersburgh for several weeks before the recent outbreak of cholera, ought to have had less weight with the London Board of Health, particularly as that gentleman seems to have indulged in a sort of philosophical mysticism, which induced a frame of mind nearly allied to blind credulity. This is the only explanation I can give of Dr. Adair Crawford's recording that the crows in St. Petersburgh had forsaken some of their usual roosting-places in the public gardens of the city and suburbs, where they are generally found in great numbers, and had flown to the nearest high grounds. The learned doctor does not inform us whether this migration was preceded by any unusual mortality in the rookery. If no such mortality took place, then I have too much respect for the wisdom of crows to believe that this alleged flight had any connexion with the cholera or deleterious changes in the atmosphere; but we are, I suppose, to go back to the

(*a*) Doctor Byrne, of New York, has published a most interesting work, containing numerous proofs of the contagiousness of cholera, taken from observations made during the prevalence of this disease in America, some of which I hope to lay before the readers of this Journal on a future occasion.

Pagan superstition, and assign to Doctor Crawford the duties of an augur,—" observans quæ signa ferant, quò tendere pergant."

Case of Intestinal Calculus. By Alexander Harriss, L. R. C. S. I., Drumlish Dispensary, Co. Longford.

July 16, 1847, I was called upon to visit Margaret Breslin, a poor woman aged 54. On my arriving at her cabin I obtained the following history of her case:—About ten days previously she had been at a village some miles distant from her abode; on her reaching home she felt much fatigued, and also complained of a pain and soreness in the lower part of her left side, which she attributed to her having carried a heavy basket. Believing her illness to be occasioned by the above, she went to bed, hoping to be relieved in the morning. In this, she was disappointed, as she gradually became worse, until the fourth day, when she was forced to take to her bed, and keep on her back, with the thighs flexed on the abdomen, as that was the only posture by which she obtained relief. She said she was the mother of nine children, all living and healthy, and that she always enjoyed excellent health.

On examination I found her labouring under the following symptoms: great tenderness and pain in the abdomen, which was swollen and tympanitic; pulse 130, small and weak; tongue dry and white at the edges, of a dusky red in the centre; teeth covered with sordes; great thirst; eyes glassy, and pupils contracted; body covered with a cold clammy perspiration; lower extremities cold; fetid breath, which she perceived for the first time that morning, after vomiting some black stuff which she said had a very bad taste and disagreeable odour.

I then directed my attention to the spot where she complained of the pain and soreness existing to a greater degree than in any other part of the abdomen, and where she found her ailment to originate; this was in the left iliac region. I could not discover any difference between it and the other side, but she pointed to a circumscribed spot where she said all the soreness lay and began in, and that she conceited she felt a hard lump in the inside of the bowel, directly opposite to where she pointed; but from the swelling and tenderness which was present I could not feel any tumour. She had no motion from her bowels for the previous six days, although she said she had taken a large quantity of senna and salts. I prescribed a castor oil draught, directed a simple enema to be administered in three hours, and applied a blister to the seat of pain.

July 17th. Bowels still confined; pain increased, but she stated it had moved lower down; slept none; felt very weak and careless. I examined the rectum and vagina with the finger, but could not detect any tumour. Ordered her to repeat the medicine as before, and to have four grains of calomel every third hour.

July 18th. Bowels as before; pain somewhat lessened; and she

stated that she distinctly felt the lump continuing to move lower down in the passage. Calomel continued.

July 18th. No improvement, but she affirmed that the lump must be near the end of the gut, as she felt it give a roll. I examined the rectum with a glass speculum, but met with no obstruction. I then passed a large gum elastic catheter through the speculum, and, after it had traversed the rectum for about three inches beyond the speculum, it communicated the feel to my fingers as if it met with a bony substance, and I was not able to force it any further. I ordered her to omit the mercury and oil draughts, but to continue the use of the common injection. Before I left I administered a tobacco injection, the strength of half the usually prescribed form.

July 19th. No motion from the bowels; pain greatly abated. I examined the rectum with the finger, and about two and a half inches from the anus I met with the obstruction, and could also detect it for the first time through the vagina by the finger. Having brought with me a large polypus forceps, I was enabled, after some delay and difficulty, to abstract a calculus at least the size of a pigeon's egg, the long diameter of which was placed transversely across the rectum. It weighed three drachms. Ordered a common enema.

July 20th. Bowels well freed; pain, tenderness, and swelling of abdomen disappeared; gums affected by the mercury. Ordered an ounce of decoction of bark every third hour, an alum gargle, and a pint of porter daily.

The woman came several times to the dispensary, in accordance with my directions, not for medicine, but to mark the state of her health, which she said was good. The last time I saw her was in the month of March last (1848), when she stated her health to be as good as it had been for the last twenty years.

Remarks.—This case exhibits some points worthy of observation, as well from the severity of the symptoms which were present, the different diseases which some of these symptoms strongly assimilated, and the obstinate constipation of the bowels, as also as from the peculiar and known remark of the patient herself relative to the motion of the imagined lump, and the rarity of the disease itself. The severity of the symptoms were of such a nature as at once to show the practitioner that the disease under which the patient suffered was of a serious nature, and required prompt measures for its removal; for on looking back to her state as described on my first visit, it will be seen she laboured under more or less of the symptoms which characterize enteritis, peritonitis, intussusceptio, hernia, and tumours pressing internally on the intestines. The constipation was such as to withstand the strong purgative measures which were adopted, and I consider would have continued so were it not that the removal of the calculus had been so soon effected. The remark of the woman herself, as regarding the motion of the tumour, is interesting and practical, for she could distinctly point out and tell me whenever it shifted, and even trace its course externally, as if it

commenced about the termination of the sigmoid flexure of the colon, and gradually moved downwards through the rectum. Lastly, the rarity of disease; I never met with a similar case since my connexion with the profession, nor could I learn from any of my professional friends with whom I have conversed about it, that they had met with such another.

In appearance the stone is white and soft externally; it feels as if it had been saturated with oil, and when broken presents brilliant striæ like mica, of rather a mahogany colour in the centre.

Dr. Munro, Jun., in his Morbid Anatomy of the Human Gullet, Stomach, and Intestines, gives some cases of enterolith; and Dr. Graves, in the second volume of his Clinical Medicine, has given an account of some intestinal calculi, composed of carbonate of magnesia, which resulted from the long-continued use of that substance, in its fluid state, internally. Mr. Clift and Mr. Children have given accounts of intestinal calculi. In the cases recorded by the former, and confirmed by Dr. Wollaston, the concretions appear to have been chiefly composed of vegetable matter, with some muriate of soda, albumen, and phosphates of lime and soda; the vegetable ingredient being chiefly derived from the fibrous matter of oats, which had been extensively used in the form of oatmeal. Some intestinal calculi analysed by Dr. Marcet consisted of curdy or caseous matter.

[Dr. Harriss having forwarded us the calculus, we submitted it to Mr. Sullivan for analysis, who has furnished us with the following report:—Ed.]

"The concretion was apparently larger than a pigeon's egg, of a very light brown colour, but towards the centre of a very dark brown. It contained no distinct nucleus, although the principal part of the colouring matter appeared to exist in the centre. This was the unaltered colouring matter of the bile. It was extremely crystalline, and completely dissolved in boiling absolute alcohol, which, on cooling, deposited beautiful large plates, which proved to be pure cholesterine. I have never seen so pure a cholesterine concretion before, nor am I aware that this substance has ever been noticed as forming an *intestinal* concretion. I have, however, several times detected cholesterine in the fæces, but have not further studied the matter, although, from the curious nature of the substance, and its relation to the products of decomposition of the bile, it would be well worth investigation."

An improved Method of operating for Congenital Phymosis. By W. Colles, F. R. C. S., Surgeon to Steevens's Hospital.

On examination, in cases of congenital phymosis, we in general find the prepuce, especially in persons after the age of puberty, very much elongated, much more than sufficient to cover the glans penis; the skin forming the outer fold of the prepuce seems loose and natural until within about a quarter of an inch from its termination, where it is reflected back to cover the glans. Here there is a con-

traction, sometimes to such an extent as to present an orifice scarcely larger than that of the urethra itself.

The operations proposed to remedy this defect, and allow the exposure of the glans penis, may be divided into two kinds:—1st, the entire removal of the double fold of skin or prepuce, i. e., circumcision; or 2nd, the simple incision of the prepuce in the line of the penis, over the surface of the glans.

The objections to the circumcision are:—

That the surgeon, anxious to remove sufficient integument at one incision, draws forward the skin as much as possible, hoping thus to include the folds of the prepuce within his fingers, not considering that a considerable portion of the inner fold must remain covering the glans; and then, when he amputates this portion of the integument, the outer skin recedes, sometimes half way up the penis, and a considerable portion of the inner fold remains still covering the glans. He has generally removed too much of the outer, and too little of the inner fold. Thus to complete the operation will be a tedious and painful process, but to circumcise the prepuce by any other method will be much more tedious and painful.

Another objection to complete circumcision is (as I have seen), that the wound, on healing, contracts so considerably as to cause great pain in any subsequent erection; besides this, the patient is annoyed by the constant appearance of a drop of clear, ropy mucus at the orifice of the urethra, caused by the irritation of the exposed glans, extending to the urethra, and causing this increased secretion from it; and this symptom will continue long after all other inconveniences have disappeared. If we circumcise merely this narrow band of the fold of the prepuce, it in healing will contract so as to leave the patient as bad, if not in a worse condition than before the operation.

The second plan of operation is by making an incision perpendicular to the orifice, introducing a bistoury beneath and slitting up the prepuce, on some one surface of the glans, either above or below. This will leave one or two very inconvenient flaps of skin on one or both sides of the glans, and may cause curvature of the penis in erection, from a hardened cicatrix.

The only proposal combining the advantages of the two operations, is, as far as I can recollect, the plan of M. Ricord; but this is a tedious and painful operation, and, I believe, seldom performed.

I have been in the habit, for some time, of removing the deformity by a simple and very effectual operation. I seize the edge of the prepuce, at its fold forming this narrow band, in the left hand, and holding the scalpel in the right, and at right angles with the penis, I remove a circular portion of skin, about a quarter of an inch wide. The outer fold of skin, being loose, is then drawn back on the penis, leaving the glans covered by the inner and tighter fold. I then divide this layer about half way back, more or less, slitting it up exactly in the centre, by passing a sharp-pointed bistoury under it. We have now the outer fold of skin loose, with a large circular orifice; the inner, or more contracted portion, presenting also an ori-

fice, but larger by double the perpendicular incision, which forms two angular flaps.

I then turn these flaps outwards, and by a suture attach each angle to the edge of the external skin, at about a quarter of its circumference from the frænum ; a slight suture at the frænum completes the operation. I then draw all forward so as to cover the glans.

In two or three days I remove the sutures, and generally find the wound healed, leaving a covering for the glans, differing in no respect from the natural and perfect prepuce; and in some cases it would be difficult to know that any operation had been performed, or that any had been required, on this part.

Cases of Obstruction of the Bowels, caused by Fæcal Accumulation in the Rectum. By R. B. Travers, M. R. C. S. E., Surgeon to the Aghada Union Dispensary.

Patrick Collins, aged 50, was admitted on the evening of the 13th of October, 1848, to the Cloyne dispensary, of which I then had charge. He has had no evacuation from his bowels for eight days; he is in great agony, suffering from violent bearing down, expulsive pains; complains of great distention of the rectum, feeling as if every moment he was about to have a discharge from his bowels, and is constantly making ineffectual attempts to evacuate them. His abdomen is swollen, and painful on pressure; his features are sunken, and denote great exhaustion; he is very weak, tottering as he leaves his bed to go to the close-stool; pulse 120, small and weak, and his body exhales a most nauseous odour.

On examining the rectum I found the anus open, but could not introduce my finger more than an inch into the gut, its entrance being obstructed by a mass of hardened, closely-packed fæces. He had taken several doses of purgative medicines, and had injections administered, without effect, previous to his admission.

Having ascertained the nature of the obstruction, with the handle of a common pewter spoon I gradually removed a large quantity of hard fæces from the rectum; and having cleared out the intestine as far as I could reach, I threw up two or three pints of warm water, which soon came away, bringing with it a large discharge from the bowels. I then gave him one drop of croton oil, and six grains of compound extract of colocynth, and directed that his belly should be well rubbed with turpentine liniment.

14th. Seven o'clock A. M. His bowels have been three times opened during the night; pulse 90; belly soft and free from pain on pressure: he complains of the soreness of the rectum and anus. Fomentation to the anus.

15th. Feels quite well, except for some soreness of the rectum; pulse 80; to have an oil draught; on the 18th he was discharged from the hospital.

On Wednesday, October 25, 1848, I was called to see Margaret Connor, aged 65. I found her in the following state: she has had no evacuation from her bowels for the last five days, and but very little for several days previously; she complains of acute pain and distension in the rectum, and is constantly making ineffectual efforts to evacuate her bowels; she has been unable to empty her bladder for the last two days, but the urine dribbles away when she makes a violent expulsive effort. The abdomen is swollen, and tympanitic on percussion, except in the hypogastrium, where the enlarged bladder gives a dull sound. Her skin is hot and dry; tongue dry, and covered with a brown fur; countenance flushed; pulse 112. On examining she rectum I found the anus open, its edges thick and prominent: the intestine was crammed with hardened, closely-packed fæces.

Having introduced a gum-elastic catheter, and relieved the bladder of a large quantity of dark-coloured urine, I proceeded as in the former case; and, having cleared out the rectum, I threw up about a quart of warm water, which soon came away, bringing with it the solid and gaseous contents of the lower intestine: I then gave half a drop of croton oil, and six grains of extract of colocynth in a pill. At seven o'clock, P. M., bowels twice copiously relieved, and has passed water; complains of great soreness of the rectum and anus. The anus to be fomented with cloths wrung out of warm water, and to be smeared with sweet oil.

26th. Feels quite well, except for the soreness; stupes to be continued. On the 28th she was able to leave her bed.

Patrick Coughlan, aged 50, applied at the Cloyne Dispensary on the 10th of November, 1848. He has had no discharge from his bowels for five days; he complains of violent pain in the belly, and shooting through the rectum; he is constantly forced, by a feeling of distension in the rectum, to make violent but ineffectual expulsive efforts to evacuate his bowels; his belly is tympanitic, his pulse rapid and weak, and his body exhales a most offensive odour. On examining the rectum, I found the anus widely open, its edges thick, and the rectum crammed with hardened fæces. I proceeded in this case as in the two former, and with a similar result.

William Collins, aged 15, was brought to the Cloyne Dispensary on the 27th of November, 1848. He has had no discharge from his bowels for four days; he complains of great pain and distension in the rectum, and feels a constant desire to go to stool, but without relief; he is unable to empty his bladder, but the urine dribbles away when he makes a violent effort to relieve his bowels; he has taken several doses of purgative medicines. On examining the rectum, I found a fæcal accumulation, as in the former cases, which I treated in the same way, and with a similar result.

A boy, aged 7, was admitted to the Cloyne Dispensary on the 6th December, 1848; he had no evacuation from his bowels for four days; he is suffering from violent bearing down, expulsive pains; his pulse is rapid and weak, and his tongue foul. On examination

I found the rectum full of hard fæces, which I proceeded to remove as in the former cases, and with like success.

These cases prove the importance of exploring the rectum in all cases of obstruction of the bowels, as mechanical obstacles may exist in it, which cannot be overcome by purgatives or enemata, a perseverance in the use of which will only increase the patient's danger and torment.

Enemata are useless, as no tube or pipe can be introduced through the mass; and I look upon purgatives as worse than useless, for the rectum, from its great distension, seems for the time paralysed, and is evidently unable to contract on its contents. The obstinate constipation, the bearing down, the feeling of distension of the rectum, the constant exhausting, ineffectual efforts, and the fæcal smell, form a group of symptoms which a rectal examination will prove to depend on fæcal accumulation in the gut.

The treatment of the affection is simple and direct. A pewter table-spoon, with its handle a little bent, makes an efficient instrument wherewith to quarry out the hardened mass. After the rectum has been cleared out, an operation which may occupy an hour or longer, an O'Beirne's tube, well oiled, should be introduced, and two or three pints of warm water thrown up; this will be soon followed by a free discharge from the bowels, which will give immediate relief. A drop of croton oil, with five or six grains of the extract of colocynth, in a pill, should now be given, or an ounce or two of castor oil. Leeches and fomentations may be necessary to relieve irritation or prevent inflammation of the rectum. The fæculent matter in all these cases consisted of potato-skins and undigested crust of Indian meal cakes.

MEMOIR OF SAMUEL WILMOT, M. D., F. R. C. S. I.

Surgeon to Steevens's Hospital, and formerly Professor of Surgery to the Royal College of Surgeons, &c.

Upon several occasions during the last three years we have been obliged to stay the progress of our series of biographies of the early fathers of Irish medicine, to make room for a passing notice of some recently lost friend, or eminent member of the profession, who had passed from amongst us. These were for the most part young men just entering upon their career, or those who, having got upon the ladder of preferment, were rapidly approaching a high and honourable station, when their course was arrested, often in the discharge of duties connected with their profession, or of those dictated by friendship or humanity. It is a fact, melancholy but true, that as a body of gentlemen, occupying the middle ranks of society in this country, the members of the medical profession are not long-lived. It is, therefore, a matter of some rarity when we have to record the deaths of three or four of our senior brethren, who have died since the issue

of our last Number. Of these we may mention Dr. Renny, formerly Director-General of the Army Medical Department in Ireland, the Senior Fellow, and one of the principal founders of the Irish College of Surgeons(*a*); Gerard Macklin, Esq., formerly State Surgeon, and some thirty years ago a gentleman in very great practice in this city (both of whose obituaries want of space compels us to leave over till our next publication); and Samuel Wilmot, M.D., the subject of the present memoir : all of whom have recently passed to the grave in a good old age, each having, for nearly half a century, commanded the good opinion both of the public and the profession.

Mr. Wilmot, the fourth son of John Wilmot, Esq., of the County Dublin, a gentleman of independent fortune, entered Trinity College in the year 1790, and some time after, having been called on by his father to select a profession, he unhesitatingly chose the medical. Owing, however, to a fancy of his father's, who had a strong aversion to surgery, he was, much to his vexation, obliged to confine his studies exclusively to medicine. In the course of two years his father died, when Mr. Wilmot's eldest brother, seeing that his ardour for surgery was still unabated, bound him apprentice to Mr. Hartigan, then Professor of Anatomy and Surgery in the University, and one of the Surgeons to Steevens's Hospital. The zeal and diligence in prosecuting his studies and performing his duties which characterized Mr. Wilmot during his entire professional career, soon evinced themselves in the student. At this period there were no resident pupils in the hospital, the usual duty being performed by the externs; the visit was also at seven o'clock in the morning, in winter as well as in summer; and though he resided a short distance out of the city, which afforded an additional obstacle to regular attendance, he was never known to be absent a single morning, so that his punctuality became proverbial. As a proof of the esteem entertained for, and the confidence reposed in him, by his collegiate companions, it may be stated that he was, though still a pupil, unanimously nominated to act as surgeon to a section of the College corps, embodied in 1798.

He obtained his diploma from the College of Surgeons in 1801, was shortly afterwards appointed Demonstrator of Anatomy in the University, and, during the last two years of Mr. Hartigan's life he acted as *locum tenens* for him, thus filling the professor's chair, and lecturing on surgery and anatomy combined. The successful manner in which he conveyed instruction, and the general satisfaction his conduct gave to the Board of Trinity College, are fully attested by the presentation from them of a very flattering address, accompanied by a handsome pecuniary remuneration(*b*). In 1813 Mr. Wil-

(*a*) The Council of the Irish College of Surgeons have, with becoming taste, determined to erect a testimonial to the memory of this distinguished man.

(*b*) The following is a copy of the letter presented to Dr. Wilmot:

"*Trinity College, July* 1, 1813.

"Sir,—I have much pleasure in conveying to you the unanimous resolution of the Provost and Senior Fellows, adopted at a board held yesterday, returning you their best thanks for the liberal promptitude with which you

mot took his degree of M. D. in Trinity College, and at the death of Mr. Hartigan, which occurred soon after, became a candidate for the vacant chair; and so strong were his claims considered, that he was absolutely congratulated and received by his friends as the Professor elect. The unsteady scales of fortune, however, turned, in an unexpected manner, against him, and another was elected(*a*). Though disappointed at this occurrence he had soon the compensating satisfaction that his private practice was rapidly augmenting;—indeed to the circumstance of his not becoming a professed anatomist, the Dublin School of Surgery is, in a great measure, indebted for the valuable labours of Mr. Wilmot.

After Mr. Wilmot became a Licentiate of the College of Surgeons he was appointed medical attendant to a dispensary on the north side of the city; this he held only a short time when he became one of the surgeons to the Meath-street Dispensary. This latter appointment was the means of bringing him under the notice of many wealthy merchants, who at that period inhabited the Liberty of this city; and his attendance upon them and their families brought in annually a very handsome income. He subsequently became attached, successively, to the Locke Hospital, Sir Patrick Dun's, and Cork-street Fever Hospital, which latter, however, he held for only four years.

In 1807 he was nominated by Mr. M'Evoy as his successor in Jervis-street Hospital, in conjunction with Mr. Dease. The manner in which the celebrated M'Evoy handed the appointment over to Mr. Wilmot, then almost a perfect stranger to him except by professional character, is worthy of special mention. After sending for him to his house, he addressed him nearly in the following terms:—I hope you will excuse the liberty I take in sending for you, but it is to communicate to you that I have determined to make you my suc-

undertook to continue the line of anatomical lectures, on the death of our late lamented professor; and also requesting your acceptance of 100 guineas, as a very inadequate testimony of their sense of the diligence and ability with which you discharged that laborious and important duty. Permit me to add an assurance of the high respect entertained for your personal character and your professional skill by the whole body, and by none more sincerely than by your very faithful

"Richard Graves, *Register.*"

(*a*) It was believed by many persons at the time that Dr. James Macartney, the successful candidate, was an Englishman, and a perfect stranger. He was, however, a native of the county Armagh, as indeed his decided northern accent witnessed to the day of his death. He was a student for some time in the Dublin School, and then went to London, where he afterwards distinguished himself as a writer and a lecturer upon physiology and comparative anatomy, subjects just then beginning to engage very great attention. When elected to the Professorship of Anatomy in our University, he was surgeon of the Radnorshire Militia, and Lecturer on Anatomy and Physiology at Bartholomew's Hospital, London. He was also the writer of the articles Mammalia, Birds, Fishes, Hairs, Feathers, and Incubated Egg, in Rees' Cyclopædia, and author of the Essay on Luminous Animals in the Philosophical Transactions.

cessor in Jervis-street Hospital; and I make this selection because I am convinced, from your high character, you will do justice to an institution which I dearly love, as it has been the instrument by which I have been raised on the shoulders of the poor to my present position(*a*). In this hospital, when he entered upon his duties, Mr. Wilmot's skill as a surgeon, and dexterity as an operator, and especially his adroitness as a catheterist, began to be appreciated: here he also performed all the capital operations, and his success in hernia operations was particularly marked. He also gave clinical lectures and bedside instruction in this institution, and delivered a private course of lectures on surgery,—a practice at that time but little known in this city(*b*). His connexion with Jervis-street Hospital continued till 1830, when he was succeeded by Mr. Ellis. In the year 1814 he was appointed to Steevens's Hospital, in the room of Mr. Obre; and a few years subsequently was elected one of the governors of that institution. He was one of the founders of the Park-street School of Medicine in 1824, where he lectured, along with Mr. Cusack, on surgery; and after the death of Mr. Todd he was elected Lecturer on Surgery and Anatomy in the School of the College of Surgeons in 1826, as colleague with the late Mr. Colles, and afterwards, in 1828, Professor of Surgery, when that part of the subject was separated from Anatomy. He was also consulting surgeon to the City of Dublin Hospital, and surgeon to the Hospital for Incurables; besides being attached to many institutions of minor importance, which it is needless to specify. He three times filled the office of President to our College of Surgeons.

Mr. Wilmot was, as has been already stated, a very neat and adroit operator, to which the circumstance of his being ambidextered assisted. In practice his particular *forte* was diseases of the urinary and genital organs, in the treatment of which few, if any, in this or any other part of Great Britain, excelled him. The delicacy of manipulation with which he managed such cases was only equalled by the sound practical knowledge and great experience which guided him. The greater portion of his department of the surgical course, at the College of Surgeons, consisted of the description and management of those affections. As a lecturer he had invariably a full class. Stripped of everything like ornament or rhetorical display, his quiet, easy, self-possessed manner, the soundness of his doctrines, and the immense amount of experience which he brought to bear on his subject, were attractions sufficient to excite the interest and command the attention of his auditory. As a

(*a*) With the precise form of the election to the surgeoncy of Jervis-street Hospital upon this occasion we are unacquainted, as the minute-book of the institution is not accessible to us, but we suppose the successful candidate's election was secured by the paramount influence of M'Evoy.

(*b*) The late Mr. Colles commenced his career as a lecturer by giving a private course upon the theory and practice of surgery in South King-street. It will be recollected that at that time there were no private schools in Dublin.

writer, he was remarkable for simplicity, clearness, and sound sense. He did not write much. Brought up in the school of fifty years ago, he had, with most of his colleagues, a terror of appearing in print, increased by a natural diffidence and excessive hatred of display ; yet in latter life he has often expressed to us his regret at having written so little. There was one trait of Mr. Wilmot's character which should not be forgotten by his biographer: he was, in addition to his many other virtues, thoroughly honest ; not merely honest and honourable as a man, but as a practitioner;—in him the young man found, in consultation, a *friend*, as well as an adviser. No one ever heard of Mr. Wilmot's "taking a case" from a junior practitioner.

In 1848 he resigned his Professorship, and Mr. Hargrave was elected as colleague to Mr. Porter in his stead.

He died the 7th November last, aged 75. His son, Mr. S. G. Wilmot, lately House Surgeon to Steevens's Hospital, has been elected to the surgeoncy of that institution, vacated by his father's decease.

Dr. Montgomery, in his address to the Obstetrical Society about the period of Mr. Wilmot's decease, thus spoke of his character:

"None who knew Mr. Wilmot will need to be told how much and how deservedly he was esteemed and respected, loved and honoured; not alone in the hallowed circle of domestic life and retirement, but abroad, in the wider sphere of professional duty, with the public. Few have enjoyed more entirely the affectionate regard and confidence of their patients, for the conviction could not be resisted that in him they had combined the skilful physician and the kind and sympathizing friend:

'Of manners gentle, of affections mild,
In wit a man, simplicity a child.'

"He possessed in an extraordinary degree the captivating grace of gentle courtesy, which, because it was the real spirit of the man, and not the well-feigned gloss of fashion,—the genuine radiance of Christian kindness and benevolence, and not the studied effort of affectation,—was given with equal readiness and cordiality to the junior student as to his own equal in years and knowledge,—to the maimed occupant of the straw pallet in the hospital, as to the titled sufferer on his bed of down.

"I have reason to know that through life he sincerely endeavoured 'so to pass through *things temporal* as finally not to lose the *things eternal*,' and 'to lay up treasures where neither moth nor rust doth corrupt.'—'They who were accustomed to rely upon' merely for professional aid will find it difficult to supply his place, to his intimates and his family his loss is irreparable, but his sons will inherit the inestimable advantage of their father's good name and example' "(*a*).

(*a*) See Southey's Memoir of Gooch.

He was indeed admired and respected by all, even by the eccentric and sarcastic Brennan, who, while he lashed the profession, good and bad, friends and foes, in his witty, and, in some respects, unrivalled epigrams, panegyrized a few whom he particularly distinguished for their honesty and modesty ; among them was Mr. Wilmot :

> " ——— the last on my roll,
> A man first on merit and modesty's scroll."

Mr. Wilmot did not write any separate or distinct work ; he contributed the following communications to the periodicals :

Case of unusual Termination of Psoas Abscess, in the Transactions of the Medical Association of Ireland, vol. ii. 1818.

A Case of Femoral Aneurism cured by tying the external Iliac Artery, in the Dublin Hospital Reports, vol. ii. 1818.

A Case of Hæmorrhagic Diathesis, in the Dublin Journal of Medical Science, vol. xix. 1841.

The History of a Case of Aneurism by Anastomosis in the Cavity of the Naris, with Remarks ; and Observations on the Formation of Organic Stricture in the Male Urethra, with some Remarks on their Consequences and Treatment : in the Dublin Quarterly Journal of Medical Science, vol. iii. 1847, and vol. v. 1848. It was Mr. Wilmot's intention to have continued this latter subject when we were deprived of his valuable services. He was engaged upon a Memoir on the Diseases of the Prostate, which will appear in our next Number.

A Course of his Lectures on Stricture and the Diseases of the Prostate, delivered at the College of Surgeons, were published in the first and second volumes of the Dublin Medical Press, for 1839, in which he also published " Cases of painful Subcutaneous Tumours." —No. for February 20, 1839.